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I. ON THE PROBABILITY OF ERROR IN EXPERIMENTAL RESEARCH.

By WILLIAM CROOKES, F.R.S., &c.

A SCIENTIFIC man engaged in any special pursuit has much difficulty in making clear, to even the scientific public, the result of his experiments. The benefit of his research may be perfectly apparent; but if his experiments have been conducted with rigour there will be a certain individual departure from a general standard in the results, which, if he merely state his conclusions, will confuse the attentive reader. Perhaps certain of the experiments were performed under better test conditions, and their numerical results are therefore more nearly correct than the results of another series of experiments. Should this be the case, to take an average of the results would yield an empirical result, deviating considerably from the truth. Yet many of our most eminent experimentalists are satisfied with recording their experiments, and leave the student of their labours in an uncomfortable uncertainty as to the exact value of the entire system of experiment.

In his "Budget of Paradoxes," Prof. De Morgan, in the consideration of the relation of facts to theory, asks the question—"What are large collections of facts for? To make theories *from*, says Bacon; to try ready-made theories *by*, says the history of discovery; it's all the same, says the idolater; nonsense, say we!" Whether, however, we take facts in subordination to theory, or the reverse, matters little for our present purpose; we have to regard the observation of facts, ascertained experimentally or otherwise, as the test of theory. But a difficulty immediately occurs to the experimentalist, and may be framed in the question—"Which, and how much, of these experimental facts am I to regard as correct, absolutely or approximately?"

Absolute correctness evidently may not be expected of any of the human senses, since the absence of error would include perception and correct estimation of the most minute deviation from the absolute standard. Between even this deviation and the absolute standard there are an infinite number of results that may be obtained. Approximate correctness is all, then, that the experimentalist has the probability of attaining. So that, given a series of experimental results, there remains the decision of two points—(1), the determination of the standard; and (2), the probable error of the assumed standard.

There are many engaged in experimental research, even in research of a relatively high order, to whom the methods of determining these two points are unknown. To these I shall endeavour to explain the laws of probability as they have been laid down by eminent mathematicians. To those (and I am afraid their number is not legion) acquainted with these laws, I can offer illustrations only of the application of these laws. Without a due consideration of these principles astronomy could not claim its character of exactness; and there appears no reason why the physical and chemical sciences should not, as means of observation increase in delicacy, attain to the rank and estimation of exact sciences. Our chronographs measure easily to the 1-100,000th of a second of time; our balances turn with a fragment of a hair weighing 1-10,000th of a grain; the results of electrical experiments have been obtained varying only 5 in the 1000. With this exactness, surely we may think it carelessness that does not ascertain the closest approximation to accuracy, as well as the limit of error to be allowed this approximation.

The subject is a most subtle one. It may be defined as the best mode of combining observations so as to yield the most trustworthy mean; and in this light I am unable to mention any work affording so popular and so profound discussion as the little volume, by Prof. De Morgan, entitled "An Essay on Probabilities." The author shows how the observer may *measure* the degree of confidence to which the *average* of any series of observations is entitled. Thus, taking his own example,—that of fifteen observations giving the following results: 722, 933, 1033, 917, 1311, 1089, 972, 1294, 967, 1344, 1250, 744, 1309, 858, 1029,—if the average, or the arithmetic mean, of all the observations be 1051, the theory does not assist the observer in estimating the probability of this average being *true*. For *true*, in the absolute sense of the word, it, in all probability, is not, and therefore no theory is needed to assist in drawing that conclusion. But let any definite departure from truth be named, then the theory enables the observer to determine the degree of likelihood that his average is within such limit. Thus it is found that the probability of the average of 1051 being within 50 of the truth (whatever the truth may really be) is 0·66; and therefore the observer should contemplate the possibility of his average being within 50 of the truth, or, which is the same thing, that the truth lies somewhere between the limits 1051 ± 50. This he would assume with the same degree of confidence, neither more nor less, that he would yield to a witness who is known to speak truth 66 times and falsehood 34 times out of every hundred.

If the calculation had been made for the limits 1051 ± 10, the resulting probability would have been much less than 0·66; and if for 1051 ± 100, the resulting probability would have been much greater. This is in accordance with common sense.

It must also be very important to the observer, when he has made different *sets* of observations, to know how best to combine their respective averages. For both purposes the average of the set, or of each, may be "weighted" by means of the formula—

$$W = \frac{n^2}{2\Sigma e^2},$$

where n = the number of observations and Σe^2 = the sum of the squares of the successive differences obtained by subtracting each observation from the arithmetic mean (average) of the whole.

The term *weight* is almost self-explanatory. Of a series of observations there may be one which the observer considers to have been obtained under more favourable conditions than the others, and to which, in the balance of judgment, he should accord greater "weight." For if we suppose a satisfactory experiment to give 5, and one of unequal weight 6, it would be obviously unfair to take the average as $5\frac{1}{2}$; but it would be more reasonable to give the result 5 the advantage of supposing it to have occurred, say, three times to the occurrence of the result of 6 once. This would be giving the observations 5 and 6 the weights of 3 and 1, and the average would be $5\frac{1}{4}$. Such a mode of reasoning gave rise, before mathematicians had constructed the theory of probabilities, to a rule for finding the average, which may be quoted as follows:—Weigh every observation, multiply it by its weight, take the sum of the products, and divide this sum by the sum of the weights. But it was found that the theory of probabilities dictated a variation of this rule, embodied in the formula previously stated. This formula may be understood in words as follows:—Square the number of observations, and divide this product by twice the sum of the squares of the errors.

Having now defined the term "weight," we have to trace the meaning of the terms *mean risk* and *probable error*.

If we consider positive and negative errors as equally probable, they will balance each other, so that the average of positive or of negative errors will be equal. We thus arrive at the meaning of the term *average error*, and can proceed easily to the determination of the *mean risk;* and as the mean risk of positive error is the average positive error, the mean risk of negative error the average negative error, the mean risk may be taken as half the average error. Representing the mean risk by m, and the weight by w, we have—

$$\text{Mean risk} = \frac{200}{709\sqrt{w}},$$

$$\text{or more nearly,} = \frac{0{\cdot}2820953}{\sqrt{w}}.$$

$$\text{Or, mean risk} = \text{prob. error} \times 0{\cdot}591473.$$

The *probable error* is that error for which there are equal chances of exceeding or of not attaining. For instance, suppose the chances are equal that the error is included between 0 and 2, or that it should exceed 2, assigning this as the limit of error, then, of course, for any number greater than (say) 10 the chances are in favour of the error being included within the number. It has been calculated that—

$$\text{The probable error} = \frac{62}{130\sqrt{w}},$$

$$\text{or more nearly,} = \frac{0{\cdot}476936}{\sqrt{w}}.$$

$$\text{Or, the probable error} = m\,1{\cdot}690694.$$

Calling the mean risk m, the probable error p, and the weight w, we have from the preceding reasoning the following formulæ:—

$$w = \frac{113}{1420m^2}.$$

$$w = \frac{5}{22p^2} = \frac{0{\cdot}227468}{p^2}.$$

From the foregoing formulæ, when either the weight, probable error, or mean risk is given, the other two can be determined. As we are capable, in most scientific observations, of so adjusting our instrumental means that the errors may have positive and negative signs, so are we now in a position to verify these observations, or select those most closely approximate to the truth.

But there have been tabulated the values of the celebrated definite integral—

$$H_t = \frac{2}{\sqrt{\pi}} \cdot \int_0^t e^{-t^2} . dx ;$$

and from these tabulated values (for the extension of which we are again indebted to Prof. De Morgan) we may, with much less trouble and more accuracy, arrive at the desired result. The values have been arranged in two tables: the columns of Table I. are headed thus:—

t.	H.	Δ.	Δ^2.
0·50	0·52049 99	874 38	8 88
2·17	0·99785 11	9 96	44
2·68	0·99984 94	84	5

t represents every hundredth of a unit from 0 to 2.

H represents the values of the area enclosed by an asymptote, this asymptote continually approaching but never reaching the abscissa, the whole of the enclosed area forming one square unit.

Δ represents the differences of these values.

Δ^2 represents the differences of these differences.

In Table II. are three columns only, t, Δ, and K,—a modification of H,—headed thus:—

t.	K.	Δ.
4·6	0·99808	40

Let us now take an illustration. There are ten observations of which the arithmetic mean is—

$$\alpha = 200{\cdot}01577.$$

The sum of the squares of the ten differences between a and each individual observation is—

$$\Sigma e^2 = 0{\cdot}00000007.$$

Hence the weight of a is—

$$w = \frac{\text{(number of observations)}}{2\Sigma e^2} = \frac{100}{0{\cdot}00000014} = 715000000.$$

The largeness of this figure indicates the high degree of probability that a is very near to the true value sought. But the query may be put—What is *the* true value? It will be seen that the question does not admit of absolute answer, and for the following reasons:—One of the observations was (say) as low as 200·0156; one was as high as (say) 200·0159. Further observations might have yielded results still more extreme. But assuming they would not, still the number of possible values between 200·0156 and 200·0159 is infinite. The arithmetic mean a is but one of these, and although more likely than any other that could be named, it is not more likely than one or other of all the possible values. The odds are more than 1000 to 1 that a is not the truth; but they are also more than 1000 to 1 that a is very near the truth. The question—How near?—cannot be answered. Alter the question to—What is the probability that the truth is comprised within the limits $a \pm k$?—and the answer may easily be given, however small k may be. Thus, if $k = 0·0001$. In other words, if the question be—What is the degree of likelihood that the truth lies between 200·0157 and 200·0159?—the answer is given by the formulæ—

$$\pi = \mathrm{H}_t,$$
$$t = k\sqrt{w},$$

where k, 0·0001,

„ $w = 715000000$, and as log. $w = 8·854$, log. $\sqrt{w} = 4·427$,

„ $\sqrt{w} = 26800$.

Then $t = k\sqrt{w} = 2·68$, and from Table I.—

$$\pi = \mathrm{H}_{2·68} = 0·99985.$$

The result, 0·99985, is so near to unity (the measure of certainty) that for every practical purpose it may be considered certain that the truth is really comprehended within the limits named.

Take six other observations, the arithmetic mean of which is—

$$a = 203·870;$$

and the sums of the squares of the six differences between a and each separate observation is—

$$\Sigma e^2 = 0·71515.$$

Then the weight of a is—

$$\frac{36}{2\Sigma e^2} = \frac{18}{\Sigma e^2} = \frac{18}{0·71515} = 25·2.$$

The smallness of this figure, contrasted with the 715000000 of the preceding example, indicates a comparatively low degree of probability that the true number is comprised within the limits $\pm k$, when k is very small. For instance, suppose the question to be—What is the likelihood that the true number is comprised between 203·77 and 203·97? As before, the probability will be determined by the given formula.

If $k = 0{\cdot}100$,
$w = 25{\cdot}200$,
$\sqrt{w} = 5{\cdot}020$,
$t = 0{\cdot}502$,
$\pi = H_{0{\cdot}502} = 0{\cdot}52$.

This result, 0·52, is just intermediate between unity or certainty and zero, the lowest degree of probability, otherwise denominated impossibility. Hence, that the true number is within the limits stated is just as likely as the throwing of Head with a single halfpenny, but not more likely.

I will now submit a practical application to the reader.

The value of the results obtained during any series of experiments must of course vary with the care taken in the performance of the individual experiments. In support of this view I have, in the practical application of the laws I here endeavour to simplify, taken the utmost pains to ensure accuracy. The application is the determination of the atomic weight of thallium; and I shall first enumerate the means (not usually employed) by which I deem accuracy to have been ensured, and then proceed to evolve the results.

With a metal of so high an atomic weight (203·642) as thallium, errors and inaccuracies comparatively trivial with elements of low atomic weight, are magnified into alarming proportions. Impurity of the reagents employed, imperfect manipulation, but, more than any, the inaccuracies arising during the weighing from the omission of the corrections required by temperature, pressure, &c.,—all these influences must be eliminated in the determination of an atomic weight.

The atomic weight was derived by two methods:—First, by taking a known quantity of metallic thallium, dissolving it in nitric acid, and weighing the nitrate of thallium produced. Secondly, in dissolving known quantities of sulphate of thallium in water, and ascertaining how much nitrate of barium is necessary to precipitate the sulphuric acid as sulphate of barium.

There were also two methods of weighing: one in air, at ordinary pressure and temperature, and one in a highly rarefied atmosphere. For the first method a balance was employed, made expressly for the work by Messrs. Keissler and Neu, which will indicate clearly a difference of 0·0001 of a grain when loaded with 1000 grains in each pan. For the second method of weighing a balance was employed which I term a *vacuum* balance. It was made by Oertling, and is a duplicate of the first, but it is enclosed in a cast-iron case, connected with an air-pump, and arranged for the weighing to take place in air of any desired density. The best manner in which to use such a balance as this is to introduce a certain approximate weight, and then to alter the pressure of the air until the balance shows equilibrium. Two weighings, at different degrees of atmospheric pressure, varying by a considerable interval, give data upon which to calculate what the weight would be in a perfect vacuum. For the full elucidation of the formulæ employed, for the method of adjusting the standard grain-weights according to their value *in vacuo*, and for the preparation of the glass apparatus and the pure reagents, I must refer the reader to my paper *in extenso*, contributed to the Royal Society.* But I may, from the abstract of that paper, collect the results of a series of the weighings. They were as follows:—

The weight of the glass + thallium.
The weight of the glass + nitrate of thallium.
The weight of the glass alone.

Grs.
True weight of thallium *in vacuo* . = 183·790232
True weight of nitrate of thallium *in vacuo* . . . . = 239·646066
True weight of glass . . . . . = 766·133831
(*a*) Weight of thallium according to true value of weights in air = 183·783921
(*b*) Weight of nitrate of thallium in air (1005·425937 — 765·814578) = 239·611359
(*c*) Weight of glass, &c., in air . = 765·814578
Weights employed to balance (*a*) . = 183·8099
Weights employed to balance (*b*) (1005·4364 — 765·8081) . . = 239·6283
Weights employed to balance (*c*) . = 765·8081

From these data the atomic weight can be deduced by simple proportion, but the results of the statements of the proportion are absolute only if the atomic weights of nitrogen and oxygen are correct. The determinations of Prof. Stas show that the atomic weights of nitrogen and oxygen should be represented by $N = 14·009$, and $\Theta_3 = 47·880$, instead of $N = 14$ and $O = 16$, as hitherto more generally held. The equivalent of nitric acid thus becomes $N\Theta_3 = 61·889$, instead of the old equivalent $NO_6 = 62$. Taking as data Prof. Stas's determination of the atomic weights of nitrogen and oxygen, and the weights *in vacuo*, the quantity of nitric acid required to convert the thallium into nitrate is—

$$(239{\cdot}646066-183{\cdot}790232=)55{\cdot}855834 \text{ grs.}$$

We have then the proportion—

Weight of Nitric Acid.		Weight of Thallium.		Atomic Weight of Nitric Acid.		Atomic Weight of Thallium.
55·855834	:	183·790232	::	61·889	:	x;

$$\therefore x=203{\cdot}642.$$

Substituting the old equivalents we obtain—

$$55{\cdot}855834 \quad : \quad 183{\cdot}790232 \quad :: \quad 62 \quad : \quad 204{\cdot}007$$

as the atomic weight; but I cannot admit this number to be so nearly correct as 203·642.

If we take the corrected weighings in air of ordinary density, we have, with $N\Theta_3=61{\cdot}889$,

203·738.

With $NO_6=62$,

204·103.

Accepting the uncorrected weights, observed in air, we have, with $N\Theta_3=61{\cdot}889$,

203·162.

With $NO_6=62$,

204·165.

The error of the last deduction is +0·523, a sufficiently large number to show the inutility of the application of the theory of probability until every care has been taken to eliminate the errors arising from inaccuracies. As I have stated, in the paper to which I have referred, the largeness of these errors has an immediate bearing upon quantitative analysis, for it is shown that, from data ordinarily given, very varying results may be obtained. Chemists have to deal with much smaller quantities than a quarter per cent, particularly in organic analysis, where so wide a difference from the truth may lead to very erroneous reasoning.

Pass we now to the application of the theory of probabilities to ten results of the most trustworthy weighings. These, with $N\Theta_3=61{\cdot}889$, are as in Table I.—

Tabulating the results of the determinations, with the view to ascertain severally their degree of approximation to the arithmetic mean (Table II.)—

The arithmetic mean of the ten observations is—

$$a=\frac{2036{\cdot}424}{10}=203{\cdot}642.$$

TABLE I.

Determination.	True Weights *in vacuo.*			Calculated Atomic Weight from these Data.
	Weight of Thallium taken.	Weight of Nitrate of Thallium + Glass.	Weight of Glass.	
	Grs.	Grs.	Grs.	Grs.
A.	497·972995	1121·851852	472·557319	203·666
B.	293·193507	1111·387014	729·082713	203·628
C.	288·562777	971·214142	594·949719	203·632
D.	324·963740	1142·569408	718·849078	203·649
*E.	183·790232	1005·366796	766·133831	203·642
F.	190·842532	997·334615	748·491271	203·636
G.	195·544324	1022·176679	767·203451	203·639
H.	201·856345	1013·480135	750·332401	203·650
I.	295·683523	1153·947672	768·403621	203·644
K.	299·203036	1159·870052	769·734201	203·638

TABLE II.

A.	203·666	+0·024
B.	203·628	−0·014
C.	203·632	−0·010
D.	203·649	+0·007
E.	203·642	+0·000
F.	203·636	−0·006
G.	203·639	−0·003
H.	203·650	+0·008
I.	203·644	+0·002
K.	203·638	−0·004

The sum of the squares of the differences is—

0·000576
0·000064
0·000049
0·000004
0·000000
0·000009
0·000016
0·000036
0·000100
0·000196

$$\Sigma e^2 = 0{\cdot}001050$$

Therefore $2\Sigma e^2 = 0{\cdot}0021$; and the weight ($w$) of a is—

$$= \frac{100}{0{\cdot}0021} = 47619.$$

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* Fully illustrated in the Paper.

We have then, from the formula,—

$$\text{The probable error} = \frac{62}{130\sqrt{w}},$$

$$\frac{62}{130 \times 218} = \log.\ 62 - (\log.\ 130 + \log.\ 218) =$$
$$\log.\ 0{\cdot}0022,$$

the number 0·0022 as the probable error. Or by means of the tables calculated from the definite integral we can arrive at a similar result. Thus—"What is the probability that the truth is comprised within the limits $a \pm k$?" If $k = 0{\cdot}01$; and $\pi = H$

$$t = k\sqrt{w};$$

$$\begin{aligned} \text{then }\ w &= 47619, \\ \sqrt{w} &= 218, \\ t = k\sqrt{w} &= 2{\cdot}18, \text{ and} \\ \pi = H_{2{\cdot}18} &= 0{\cdot}99795, \end{aligned}$$

so near to unity, the measure of certainty, that the number 203·642 may, for all practical purposes, be regarded as the absolute truth. From the second table we can also obtain like results by entering with t. We obtain the argument from the formula—

$$\frac{k}{\text{probable error}} = t.$$

Therefore $\frac{0{\cdot}01}{0{\cdot}0022} = 4{\cdot}6 = t$, to which corresponds $k = 0{\cdot}99808$. There can remain no reasonable doubt, then, that the atomic weight of thallium is $= 203{\cdot}642$.

As simply as I am able, I have endeavoured to show the application of the theory of probabilities to the judgment of error, and the evaluation of the amount of accuracy in experimental research. The subject has, I think, been involved with undue difficulty. Perhaps it has hitherto been generally held that the results of experimental research have not been sufficiently accurate to permit the refinement; but I must express an opinion quite opposed. Yet I would suggest that, in all kinds of delicate weighings, the effects of temperature and pressure of the atmosphere be taken into consideration. Let me make my meaning clear by an example. There are given to be weighed, let us say, 800 grains of water in 200 grains of glass. First arises the question,—Shall we employ brass or platinum weights for our determination? We shall presently see the difference that would result, in the determination of the absolute weight of the glass and water, from the result of our choice. A brass weight of 1000 grains will displace 0·1462 grain of air; an equivalent platinum weight 0·058271 grain of air. The 1000 grains of glass and water displace 1·9736 grains of air, so that their absolute weight is 1001·9736 grains. Now the glass and water balanced by the brass weight would give, less the air displaced by the weight, 1001·8274 grains as the true value of the water and glass; while 1001·9736 grains, less 0·058271 grain, give 1001·915329 grains as the value to be ascertained. So, supposing the barometrical pressure to remain constantly at 760 m.m., we have an error of 1·8276 grains per 1000 in weighing with brass weights uncorrected in air, and 1·915329 grains per 1000 with platinum weights at the same barometric pressure. But we know that the barometer does not always record the same pressure. What, then, will be the result of its variation?—the variation, of course, of the weight of air displaced. Now a litre of dry air (at Greenwich), at 760 m.m. pressure and 0° C., weighs 1·293561 grms., and its weight will be proportionately lower at lower pressures. At 740 m.m. the weight of air displaced by water and apparatus will be 1·9216 grains, and at 715 m.m. 1·8890 grains. The weight of air displaced by the brass and by the platinum weight also decreases proportionately. So that, weighing with the brass weight, we have, at 740 m.m., an error of 1·7792 grains on the 1000, and at 715 m.m. an error of 1·7505 grains. With platinum weights we have, at 740 m.m., 1·864863 grains error, and 1·834334 grains at 715 m.m. These discrepancies are too important to be disregarded. For suppose our weighings to have taken place on different days, at different pressures which were not noted, we should have serious error; and the error would be increased with a specifically lighter fluid than water.

Chemists are aware how greatly an error of similar character would influence the determination of the amount of carbonic acid and of water yielded by an organic body under combustion. Suppose the potash bulbs employed in the analysis to weigh 600 grains, there would be displaced 0·366 of a grain of air at 760 m.m. pressure, 0·327 grain at 740 m.m., and 0·316 grain at 715 m.m. Thus if weighings were made at 715 m.m. and at 760 m.m., there would be an increase of weight of 0·02 grain; and this, if 3·5 grains of the organic compound were under analysis, would give an error of 0·6 per cent. Similarly with a chloride of calcium tube, weighing, with its contents, 350 grains, there would be an error which—with the error in the estimation of the carbonic acid—would give a total error of nearly 1 per cent. Of the effect and importance of such an error it is unnecessary to speak; to all in the least acquainted with analytical research there will appear full reason for the more careful study of the subject.*

These facts clearly show the necessity,—first, of great care and great delicacy in all manipulation connected with experimental research; secondly, of carefully "weighing" the individual merit of each result, and its relative merit in the series of results. How this may be effected I have endeavoured to explain; and I think that there would be no series of observations (to which this or an analogous method has not been applied) but would benefit by the application. The application should of course proceed from the experimentalist himself, but there are many series of results, the members of which have been obtained by different processes, that would be rendered still more practically useful by an evaluation according to some one of the principles of the theory of probabilities. Perhaps in future years the theory may be universally understood, and it will not be required to revert to the elements of the Science.

II. GOLD-MINES AND MILLING OF GILPIN COUNTY, COLORADO, UNITED STATES.

By JAMES DOUGLAS, Quebec.

FOURTEEN years ago a party of miners detected gold in Dry Creek and other spots near the present town of Denver. The news spread; a rush ensued, and exploration was rapidly carried from the plains up the gorges of the Rocky Mountains. Before 1859 had closed, the gulches round Central City, 40 miles distant from Denver, were swarming with gold-diggers; and mining had also commenced on the rich surface quartz of the lodes, whose disintegrated *débris* had supplied the gold that enriched the neighbouring valleys.

In what is now Gilpin County, and within an area whose centre is Central City, and radius about 1½ miles, was discovered, before 1863, a gold-bearing lode at almost every hundred feet; and many of these lodes were yielding gold and matter for exaggeration so abundantly that American brokers were enabled to form, in the cities of the east, no less than 186 public gold-mining companies. The companies generally possessed capital enough to build a mill, but before the mill was running it in many cases happened that the surface rock, which yielded its gold to mercury, was exhausted, and after a few experiments the mill was stopped; and mill and mine have remained closed ever since. A few mines, however, rich enough to bear the loss of from three-fourths to two-fifths of their produce in the mill, have remained open, to testify to the extraordinary richness of the district. As the mills existed they have continued to be used, despite the defects of their work; but unless some better system be introduced mining must languish, for no mines can long sustain such waste.

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* In the course of my experiments with the delicate apparatus employed in this research, I have noticed some curious effects of the action of heat upon gravitating bodies. Led to pursue the investigation with specially constructed apparatus, in air and *in vacuo*, I hope, at no distant date, to bring forward some results.

The present article is a contribution towards the solution of the question, which, as it involves the saving or loss of several million dollars' worth annually of gold, silver, and copper, is well worthy the attention of metallurgists. So abundant is the ore that were mining conducted systematically, and the product of the mines utilised, Gilpin County would probably yield more value in mineral than any district of equal size in the world.

The country rock is granitic, with some gneissic varieties. The lodes have a general E. and W. course, and dip almost vertically. They are very free from faults, and many of them can be traced, running with remarkable regularity, for long distances; but the productive portion rarely exceeds 4000 feet. The deepest shaft in any of them is only 700 feet, and there are few others deeper than 500 feet: it is therefore impossible to predict what their character will continue to be, and whether the gold yield will be permanent; and the changes which have taken place in certain of the lodes, at different depths, are too inconsistent with one another to allow of any deductions being drawn from them. The structure of the lodes is very characteristic of fissure-veins. The walls are usually distinct, and marked often with well-polished schlicken sides. A clay sewage, then a band of almost pure iron and copper pyrites, intermixed with small quantities of blende and galena, or of blende and galena alone, or of all these sulphurets mixed in almost equal proportions, occurs on one or both sides, while the centre of the lode is composed—where the lode is rich—of a gangue of decomposed quartz or felspar, carrying more or less of the same sulphurets. The solid sulphurets of iron and copper, known as No. I., or smelting ore, usually yield to the miner from 60 to 80 dollars a ton. The copper pyrites carries most gold, and the fine-grained iron pyrites more than the coarse, distinctly cubical, variety. The blende is also associated with gold, and in some mines is the principal vehicle of it, and the galena is invariably argentiferous. This rich ore is always sold to the smelter, as it refuses to give even as large a percentage of its gold to mercury as the less concentrated ores of the body of the lode, where the gold seems to be in a freer form. The second class ore, in first class mines, will usually carry—

1·4 ozs. of gold,
5·6 ozs. of silver,
2·8 per cent of copper.

It is always treated in stamp-mills where battery amalgamation is employed, and not over 33 per cent of the above-named valuable constituents of the ore recovered.*

The proportion of No. I. ore to No. II. ore rarely exceeds one-tenth, and in most mines the quantity is too small to make it worth while effecting any separation.

The width of the lodes runs from 18 inches to 10 to 12 feet. An average width of the really productive lodes may be set down at 3 feet, but they are all subject to contractions and expansions, sometimes pinching to a mere thread, at other times bulging into enormous bunches. Nor are any of the lodes consistently productive. The mineralogical portions are said to run in chimneys, which are interrupted by streaks of poor or altogether barren rock. The term "chimney" has been borrowed from California, but is not applicable in Colorado, as the rich ground does not form continuous vertical streaks, alternating with vertical streaks of barren rock, but irregular regions of rich ore, merging vertically and horizontally into poorer ground. The term "cap" is applied indiscriminately to merely lean and altogether barren ground. Of the latter there is comparatively little; and as the former includes all ore that will not yield 20 dollars of gold to the ton, much that is now left standing in the mines, it is to be hoped, will some day or other be removed with advantage.

Unfortunately the mining in Gilpin County has been as faulty as the milling, owing chiefly to two causes:—

I. The subdivision of the lodes into very small claims.

II. The failure of the companies very generally to work their claims,—which has led to the mines being either let or worked on tribute. In either case the miner, having no interest in the property, aims only at extracting as much ore as he can during the term of his lease, without regard to the future of the mine.

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* Mr. ALBERT REICHENECKER, in the Berg-Hüttenmännische Zeitung, reproduced in RAYMOND's Report on Mines and Mining for 1870, p. 360.

I. To what a degree the subdivision of the lodes has been carried may be judged from the following enumeration of the claims on some of the principal lodes in the district. The list is taken from Mr. G. W. Baker's pamphlet on the treatment of gold ores in Gilpin County, Colorado.

On the Gregory Lode—

	Feet.
The Black-Hawk Co. owns	500
,, Consolidated Gregory Co. owns . .	500
,, Marragansett Co. owns	400
,, Rocky Mountain Co. owns . . .	200
,, Benton Co. owns	600
,, Russell (Extension) Co. owns . .	300
,, Briggs Co. owns	250
,, Smith and Parmlee Co. owns . .	1100
,, New York (Extension) Co. owns .	250
,, United States Co. owns	250
,, Manhattan . , ...	400
	4750

On the Bobtail Lode—

	Feet.
The Bobtail Co. owns	433⅓
,, Stirling Co. owns	60½
,, Brastow Co. owns	62¾
,, Sensenderfer Co. owns	128
Private owners in small claims own from	700 to 800
	1483

On the Bates Lode—

	Feet.
The Rocky Mountain Co. owns . . .	250
,, Bates and Baxter's Co. owns . .	300
,, Union Co. owns	200
,, Loker Co. owns	400
,, Gregory Co. owns	100
Private persons	300
	1550

These three lodes have been the most productive in the district, and the most diligently worked. The Bobtail has, it is estimated, yielded about 3,000,000 dollars' worth of bullion,—no insignificant yield, considering how short is the really metalliferous portion of the lode and shallow the shafts, few exceeding 400 feet. These lodes carry less galena and blende than most others, and a larger percentage of copper. These three lodes run almost parallel, and so close together that the slight convergence in their course westward has given rise to the conjecture that they unite to form the Mammoth Lode, which can be traced for about 3000 feet from a point a little west of the known westernly limits of the Bobtail. This lode is likewise divided into a number of small claims, the longest of those owned by companies being 400 feet. The lode is wide, and the ore highly charged with iron pyrites; strange to say, almost free of gold. But proceeding further west, and crossing a ravine known as Spring Gulch, we reach a group of parallel lodes so similar in course and dip to the Gregory, Bobtail, and Bates, that, though undetected in Spring Gulch, one cannot but look upon them as a continuation of those three lodes, or, if they are really united in the Mammoth, of this lode again split up into several branches. The most notable of this group is the Burroughs Lode, on Quartz Hill, on which—

	Feet.
The Ophir Co. owns	462
,, Gilpin Co. owns	262½
,, Colorado Co. owns	200
,, Burroughs Co. owns	200
,, Cooper Co. owns	50
,, Hardeslis Co. owns	200
,, Pacific National Co. owns	550
,, First National Co. owns	600
,, Gold Hill Co. owns	70
,, Quartz Hill Co. owns	30

This group and the lodes of the neighbouring Nevada district are, as a rule, poorer in gold and copper, but richer in argentiferous galena, than the preceding.*

The ill effects of such a subdivision it is not difficult to conceive. As every proprietor sinks one or more shafts, a vast amount of unnecessarily expensive work is done. Moreover, the chances of individual failure are greatly increased; for unless the owner be fortunate enough to hit a rich chimney of ore, which sinks vertically without interruption, when he runs out of good ground he is sure to be in unproductive ground from end to end of his claim, and therefore as sure to fail financially. The evil is now, however, curing itself. As the mines have been sunk the water has become more and more troublesome, and combination has been forced upon the owners by the refusal of some to pay their share of the expense of pumping. A process of what is termed "freezing out" has been going on for some time on the principal lodes, which, by a method hardly justifiable, is likely to lead to the desired union of interests, though at the expense of the shareholders of the companies. A mine fills with water; all returns cease; the company's affairs are liquidated, at the suit of the superintendent or some privileged creditor, for perhaps a trifling sum. The property is sold by the sheriff, before perhaps any of the shareholders in the East are aware, and the mine passes into the hands of a few men, who, if they do not acquire the adjacent claims by the same process, will work in harmony with those who do. The temporary suspension of many of the richest mines, and the consequent decrease in production of the district, is, in a measure, due to the systematic carrying out of such schemes. Some small-claim owners are, however, so fortunate that their success makes it difficult to persuade others of the evil of the subdivision system. There is an owner of some 30 feet on the Bobtail who steadily refuses to join a combination, and who cannot be either bought or sold out. He is down some 500 feet, and throughout that whole depth he has been in good pay-ground. He works for a few months, till he has taken out what gold he requires, and then *knocks off* till he needs to make another draft. As he says his gold is safer there than in any bank, he refuses either to sell or exhaust his mine. It is said that during the last spell of 9 months' work he extracted 500 lbs. weight of retort gold, value about 100,000 dols.

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* For a full and accurate description of the most important mines consult vol. iii. of the United States Geological Exploration of the 40th Parallel, On Mining Industry, by JAMES D. HAGUE.

II. The second evil, *viz.*, the failure of the companies to work their own claims, is even more detrimental to the future prosperity of the mines than that last discussed. As a rule the affairs of the companies have been grossly mismanaged. Having spent their slender capital, their superintendents have found it more conducive to their ease to let the mines on tribute or on lease than to work them. The mines are sure to yield enough to pay their salaries. The lessees work, of course, for immediate returns; hence there are few mines in Gilpin County which—through this vicious practice of "gouging," as it is termed—have not been riddled in a shocking manner. To save timber the old road-ways have been removed; the stopes, if filled with poor ground, are blocked for hundreds of feet, so that it is generally impossible, without great cost, to examine the ground left standing, or, if the stopes be empty, they are vast caverns, with the roof so feebly supported by a few slender props that it is with greatest risk one enters them. These defects of the past—due, in chief measure, to the faults of the superintendents, though in part to the ignorance of the miner, who went to his task from a farm in the East or cattle-grazing on the plains without any previous knowledge, far less education—will, it is to be hoped, not disfigure future operations. When better methods of treating the ore are introduced, the miner will wish to reach the once unremunerative but now valuable ground left standing, and the difficulty and expense of doing so will teach him that it would have been cheaper to have properly opened and kept open his mine from the first.

METHODS OF TREATMENT.

Battery Amalgamation.—At the outset of mining, 12 and 14 years ago, when the rich surface quartz carried free gold abundantly, stamp-batteries, supplied with riffles and such appliances for catching the free gold, were employed. When the sulphurets were reached these failed altogether to secure the precious metal, and amalgamated copper plates supplanted the riffles. But it was some time before the mill-men understood the necessity of thoroughly cleansing and amalgamating the plates. To arrest the sulphurets blankets are, in some mills, placed below the amalgamated plates, and the blanketings ground in pans, the mercury in the concentrate sufficing for the amalgamation of the small quantity of gold thus saved. The tailings are frequently further concentrated in tins—those called "hand-buddles." Round buddles have been tried, but found too slow, and to require more attention than the rough impatient workman will bestow.

The stamps are run slowly, never exceeding 30 strokes,—a higher speed interfering with the battery amalgamation, by discharging on the plates too great a volume of water and slime. Amalgamated copper plates are fixed within the battery, under the charging and discharging openings, and form an apron in front of the discharge 10 feet to 12 feet long, and set at an angle of 10° to 14°. Mercury is added every two hours, through the charging-slit, in quantities to suit the richness of the ore: three times as much is introduced as is afterwards recovered.

The gold caught on the plates is, under the most favourable circumstances, only 40 per cent of the assay value of the ore. The quantity of silver saved is inconsiderable. The gold from the blankets, and that in the buddle concentrate, does not amount to more than 5 per cent more; so that, when treating even the most tractable of these sulphurets, battery amalgamation and tailing concentration do not secure more than 45 per cent of the gold, and therefore involve the loss of 55 per cent of the gold, and of all the silver, copper, and lead. As already stated, it is the second class ore, or that from which has been separated by hand the solid sulphurets, and from which has been thrown away stuff too poor for treatment, that is milled.

The benefit of tailing concentration is so insignificant from the simple fact that it is so carelessly and rapidly conducted, that only the very largest and heaviest particles can settle in the voluminous and swift stream of water used. Most of the tailings carry more than 1 oz. of gold to the ton, about 2 per cent of copper, and 15 per cent of iron pyrites and blende galena. The concentrate will consist of almost pure iron pyrites, very little—if any—more copper than the crude tailings contained, and seldom as much as 2 ozs. of gold. Mr. Baker gives the average contents in gold of 45 samples of tailings, from assays made by reliable assayers, Messrs. Schulz and Burlingame, at 27·86 dols. per ton,—the highest assay being 50·40 dols., the lowest 2·21 dols.; 38 samples of dressed tailings contained on an average, according to the same authorities, 42·90 dols. The heavy iron pyrites is increased four to five times by the concentration as effected now; the lighter copper pyrites, carrying the gold, is washed away into the stream. The first act of reform should doubtless be—dress the tailings from the present mills on the same system that slimes are dressed the world over.

In Gilpin County there are scattered over the hill-sides, at the mines, or in the river valleys, where water runs, but where—through perverse mismanagement—steam is nevertheless often employed as the motive power, about 70 mills, with 1300 stamps. Of these many have been idle ever since they were built, and at the best of times not more than half the number of stamps have been in operation. At the present moment, owing to the special but evanescent causes of depression already explained, there are not 300 stamps running. But in 1868-9, when Gilpin County produced 1,267,900 dols. in gold, and in 1869-70, 1,378,100 dols. in gold, the average number of stamps running throughout the year was about 400,* crushing about 100,000 tons, whose average yield per ton must have been between 10 and 12 dols., for part of the annual production came from the No. 1 ore, smelted by Prof. Hill. The absolute contents of these ores was probably 35 dols. per ton of gold and silver, 3 per cent of copper, worth say 1·50 dols. per unit, and the same percentage of lead, worth say 50 cents per unit. I attach some value to the lead, inasmuch as, though the lead scattered through the iron and copper pyrites is valueless, there are lodes yielding massive galena, and others where the galena might be separated from the other sulphurets, advantageously both to the miner and the smelter. Therefore—

	Dollars.
100,000 tons, containing 35 dols. in gold and silver, are worth	3,500,000
300,000 units of copper, at even 1·50 dols. per unit	450,000
300,000 units of lead, at 50 cents per unit	150,000
Absolute value of metals in the ore . .	4,100,000
Allow for loss in dressing, say 20 per cent	820,000
	3,280,000
Present yield with battery amalgamation and smelting of No. I. ore only (average of 2 years)	1,323,000
Saving under altered system	1,957,000

This saving would almost represent profits derived by miner and smelter; for the cost of crushing and concentration, were battery amalgamation supplanted by simple concentration and smelting of the whole produce of the mines, would be so much less than that of pulverising and amalgamating that this saving, added to the profit derived from the smelting of copper ores, concentrated as they then would be to 10 to 12 per cent, and purchased probably at 2·50 dols. per unit, would pay for the cost of treatment. Of course the miner would not receive from the smelter the full value of the gold and silver, but he would receive a higher percentage of their value than he now does, and thus the smelter and he would divide the increased profit between them.

As I shall show, the cost of mining and milling is now approximately 12 dols. per ton. The return of bullion derived from the 100,000 tons crushed confirms therefore, what is evident from other considerations, that the mines have, as a whole, merely paid the miner and mill-man their extravagant wages, without returning any profit on the capital invested in the mine. Could, however, the heavy loss now sustained be saved, the saving would be nearly clear profit. But, beside that, the very method which would save the waste would enable much of the third quality ore, now broken and raised at considerable cost, to be utilised. But to this subject I shall return after describing the

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* RAYMOND, Report for 1870, p. 294.

SMELTING WORKS NOW IN OPERATION.

One establishment—that of the Boston and Colorado Smelting Company—has for five years monopolised the smelting ores raised around Central City, and under the admirable management of Prof. Hill the enterprise has succeeded financially and metallurgically.

At present there are five calcining and three reverberatory smelting furnaces running. The ores treated are the No. I. iron and copper pyrites, and concentrated tailings, containing gold and silver. Galena ores are not sought for; but a certain amount of galena and blende is necessarily present in the mixture, and the latter is in sufficient quantity to be a source of trouble by carrying silver into the slag. A cupola furnace is employed to re-melt this zincy slag,—an expensive operation, as coke costs between 40 and 50 dols. a ton.

The fuel used in the reverberatories is wood. The calciners roast 3 tons of tailings daily, with the consumption of one cord of wood, costing 7 dols. a cord. The smelting furnaces consume about 12 cords of wood, and smelt 4 charges of 2 tons each in the twenty-four hours. The lumps of coarse ore are heap-roasted.

Professor Hill aims at getting a 40 per cent copper matt, containing 40 ozs. of gold and 400 to 600 ozs. of silver to the ton. He has always sold his matt to Vivian and Co., Swansea. His works have been of immense advantage to Gilpin County. Yet he is not in good odour with the miners generally. His scale of prices is low—lower probably than if there were a vigorous competition on the spot. Special arrangements, however, are made with good customers. But in every case the value of most of the parcels of ore is evidently guessed at, as the rough mode of sampling employed can afford only a very vague determination of their contents. The scale of prices paid is about as follows:—

Ounces of Fine Gold per Ton of 2000 lbs.	Percentage Paid of the Value of the Gold and Copper.
10	60
9	58
8	55
7	52½
6	50
5	45
4	40
3	30
2	20

The silver in the gold ores is not generally allowed for, nor is anything paid for the copper, unless in special cases.*

Professor Hill's were not the first smelting works attempted in this district; and others have failed since the establishment of his, though not in every case through defects in the method. Mr. Wm. West erected, two years ago, smelting and sulphuric acid works, with a view of removing the zinc as sulphate by Gremm's method before smelting the ores; but the concern failed through lack of capital before getting fairly under weigh. At present Mr. West is superintending works at Golden City, about half way between Denver and Central City. The works are located there in order to be near the lignite, which occurs in thick beds along the base of the mountains, and which is delivered at the works for 4·50 dols. per ton; and also because Golden City is central to the lead ores of

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* It may not be uninteresting to compare the above tariff with the price paid for gold ores, to be similarly treated, at the smelting works on the Copiopó, in Chili.

Ounces of fine Gold per cajon = 64 Spanish quintals.			Price paid per ounce.
4 ozs. per cajon, or	1 oz.	120 grs. per ton of 2000 lbs.	6·50 dols.
5 ,, ,,	1 ,,	270 ,, ,,	7·75 ,,
6 ,, ,,	1 ,,	420 ,, ,,	8·90 ,,
8 ,, ,,	2 ozs.	240 ,, ,,	9·80 ,,
10 ,, ,,	3 ,,	60 ,, ,,	10·40 ,,
12 ,, ,,	3 ,,	360 ,, ,,	10·80 ,,
14 ,, ,,	4 ,,	180 ,, ,,	11·20 ,,
16 ,, ,,	5 ,,	— ,, ,,	11·70 ,,
18 ,, ,,	5 ,,	300 ,, ,,	12·30 ,,
20 ,, ,,	6 ,,	120 ,, ,,	12·90 ,,

The same works pay for silver ores, when they contain 6 marks to the cajon, or 60·60 dols. of silver to the 64 Spanish quintals—

For 6-mark ore 1·00 dol. per mark, or 1-10th of the value of the silver.
,, 10-mark ore 3·00 dols. ,, 3-10ths ,, ,,
,, 40-mark ore 6·00 ,, ,, 6-10ths ,, ,,
,, 100-mark ore 7·25 ,, ,, 3-4ths ,, ,,

Georgetown, and of the new mineral region of Caribou, in Boulder County. They are designed to treat argentiferous galena only. In a combined calcining and smelting furnace the galena, mixed with a suitable proportion of siliceous matter, is first roasted by being moved forward from the stack to near the fireplace over a hearth 60 feet long. Immediately behind the bridge there is a depression, into which the calcined ore is drawn, and where it is fused into a pasty mass, with pyrites tailings from Central City. This mass of silicate of lead, with some galena and sulphide of iron, is smelted, with a small percentage of metallic iron, in a cupola furnace. Separating works are being put up to desilverise the lead. Although the lignite answers admirably in the calciners, Pennsylvania coke at 40 dols. a ton is the fuel consumed in the cupola.

Another market for Gilpin County ore is being made in Idaho, about six miles from Central City, over a steep lateral mountain range, where an English company is commencing the erection of furnaces in the old Whale Mill of the Spanish Bar Silver Mining Company. The works are near enough to Central to compete with Professor Hill for the richest of the gold-bearing sulphurets of that region, which it will be found necessary to mix with the more refractory ores of Idaho. At about the same distance from Idaho, but much more easily reached, because upon the banks of the same river, is the Empire City district, which will also furnish iron and copper sulphurets; but small smelting works, owned by a Swansea firm, are already in operation at this point.

Some of the richer argentiferous galena of Gilpin County finds its way to Georgetown, where Mr. J. O. Stewart has erected and is running to good profits beautifully arranged silver reduction works of the Reese River type; but to describe them would be beyond the purpose of this article. The prices he pays are somewhat more favourable than those of Prof. Hill; but it is worthy of note that the greater satisfaction of his customers arises in great measure from the accuracy with which the sample is taken, and the certainty the seller feels of knowing what his ore really contains.

PRESENT FINANCIAL POSITION OF MINING AND MILLING, AND PROPOSED ALTERATION IN THE MODE OF TREATING THE ORES.

So heavy is the loss entailed on the miner by the present system of milling, that Mr. Reichenecher computes that the "gross receipts from rock of the first class are about 36·24 per cent, and from rock of the second class about 32·13 per cent of their total assay value in gold, silver, and copper." He classes the veins now worked under two heads, the first yielding an average in gold, silver, and copper of 50·80 dols.; the second class an average of 30 dols. per ton. The first, as he shows, returns a profit to the miner; the second is worked at a loss.

Fifty tons of ore, whose contents in gold, silver, and copper is worth, say 50 dols. per ton, but in gold, which alone is saved, only 29·40 dols., will cost and yield as follows:—

	Dollars.
5 tons, worth, say 100 dols., will bring from the smelters 60 dols. per ton . .	300·00

Forty-five tons, treated in the mill, and worth in gold 29·40 dols. per ton, will yield as follows:—

	Dollars.
40 per cent of 29·40 dols. per ton, the result of battery amalgamation	529·20
4 per cent of 29·40 dols. per ton derived from the blanketings when treated in pans	52·92
Concentrated tailings, 2·25 tons at 16·94 dols.	38·12
	920·24

Cost of Mining and Milling.

	Dollars.	
Mining and carting 50 tons at 6 dols. per ton	300·00	
Milling 45 tons at 3·84 dols. . .	172·90	
Treating blanketings in pans 14½ cents per ton	6·30	
Concentrating tailings	9·81	
	—	489·01
Gross profit		431·23
From this must be deducted cost of administration, and tax of, say 18 cents per ton, amounting to, say 1·26 dols. per ton		63·09
Nett profits		368·14

Or, 7·36 dols. per ton.

Veins of the second class containing 30 dols. in gold, silver, and copper, but not over 21 dols. in gold alone, it is evident can only be worked at a loss, which is calculated by Mr. Reichenecher at 1·73 dols. per ton.

Most of the mines which have been kept open on the lodes previously enumerated are of the first class. Many, of course, have yielded much higher percentage ore than the average: many more have intermitted between poverty and richness; and there is many an isolated mine, like the California on the Flack Lode, which has for many consecutive months by its large returns effected the prosperity of the whole region. There are, moreover, first class claims held by wealthy men who can afford to wait, and who have kept them closed for years, sure of the introduction sooner or later of more economical methods of treatment. Of each there are several on the Bates Lode. The best evidence of the unparalleled richness of these mines is that, despite the loss of 66 per cent of their mineral, so many have been for years worked to advantage. A comparison of their produce with those of other gold-producing countries affords further proof of this.* The average value of 500,000 tons of Australian gold quartz was 16·78 dols.; the average value of ore raised in eight counties in California from 30 mines, including the richest, is, per ton, 23·50 dols.; while 1,760,050 tons from the Morro Velho mines, Brazil, yielded only 8·20 dols. per ton, and yet the ores of Gilpin County must yield 25 dols. in gold to cover cost of extraction and milling alone. If the character of the ore is so peculiar as to defy all known methods of economical treatment, the mines must be left to their inevitable fate. But there is no reason to apprehend such a gloomy future.

The remedy evidently lies in mechanical concentration of the second and third class ores, the abandonment altogether of battery amalgamation, and the smelting of the whole produce.

The ore should be carefully assorted by hand, and a separation made not only of first class, as at present from the poorer vein stuff, but of the iron and copper pyrites from the galena and blende.

First class ore, as at present, is fit for the furnace, and can be roasted either in heaps, or, better still, in kilns; for it is a serious waste of capital to have 100,000 to 200,000 dols. worth of ore lying in roast heaps for months, when the amount might be returned in as many weeks were kilns employed. Second and third class ore, the former of which alone is now serviceable, might both be crushed and concentrated. The gangue is generally soft and light, and easily separable from its mineral contents; and the mineral is not, as a general rule, distributed in such minute particles through the mass as to necessitate crushing finer than 1-6th to 1-8th of an inch, in order to obtain a very perfect disengagement of the one from the other. The coarse grains should be concentrated in automatic hutches. It is possible that Messrs. Huet and Geyler's hutches* would separate not only the mineral from the earthy matter, but as the toppings flow from hutch to hutch effect a certain separation of the iron and copper pyrites from the blende and galena. These hutches recommend themselves also by their compactness, and being built entirely of iron. The slime concentration would doubtless be best effected on Rittinger lateral percussion tables, which would certainly not only concentrate, but separate the concentrate into parcels of different specific gravity; but the machine requires for successful working too close attention to so many details to be efficient in the hands of Colorado ore-dressers. Buddles therefore—concave buddles for the coarse, and convex buddles for the fine slimes—would be the most suitable machines. If third class ore, which will not bear expensive carriage, is to be utilised, the concentrating works would need to be at the mines. Water could be delivered to most mines from the Consolidated Ditch. The charges are now high, but it is expected they will be reduced to 10 cents per miner's inch per day = 2274 cubic feet of water.

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* Baker's Pamphlet, p. 15.

Dry concentration is strongly advocated, but where water is accessible it will in most cases be better to adhere to the well-understood system of water dressing.

If the concentration were as carefully conducted as it is in the best establishments of England and the Continent, the result should be as favourable. In Hungary the allowance for loss is 15 per cent. Allow that it would be 20 per cent in Colorado, and that the concentrate would contain four times as much mineral as the crude ore. If, therefore, the mineral contained 1 oz. of gold and 1·5 per cent of copper, the concentrate would contain, after making allowance for loss, 4 ozs. of gold and 6 per cent of copper. I leave the silver and lead out of the calculation. If the galena and blende can be separated from the iron and copper pyrites the galena will be an additional source of profit; if not, the cost of smelting the refractory mixture and the loss of silver in the slag will be so great as to reduce notably the value of the silver.

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* Huet and Geyler, 46, Rue de la Victoire, manufacturers of the Cribles Rapides à Deux et Quatre Compartiments.

The probable cost of treating will, therefore, be—

	Dollars.
Mining and hauling 50 tons at 6 dols. per ton	300
Handpicking and concentrating 50 tons at 2 dols.	100
	400

Value of gold and silver in the concentrate—

	Dollars.	
10 tons of concentrate containing 40 ozs. of gold	800	
10 tons of concentrate containing 60 units of copper	300	
		1100

The smelter should pay for ore containing 4 ozs. of gold, 6 per cent of copper, and probably 20 to 40 ozs. of silver, at least 60 per cent of the value of the gold and 50 per cent of the value of the copper.

Therefore the receipts of the miner would be—

	Dollars.
60 per cent of the value of 40 ozs. of gold	480
50 per cent of the value of the copper	150
	630
And as the cost of producing and concentrating would be	400
The profit on 50 tons of 1 oz. gold ore would be	230

Or 4·60 dols. per ton.

This calculation supposes that there is no No. 1 ore in the vein stuff. As the custom mills charge only 3·84 dols. for stamping and amalgamating a ton of ore, the allowance of 2·00 dols. for crushing and concentrating is ample. Moreover, the smelting would doubtless be done more cheaply were there vigorous competition. But this cannot be looked for till smelters can count with certainty on a steady and abundant supply of suitable ore, which will only be forthcoming when the whole produce of the mines passes through their hands, and not the No. 1 ore only.

From mines now open 1000 tons a day of 20-dollar ore could be at once produced; and there are second class mines innumerable which under existing modes of treatment are valueless and closed, which could quadruple that yield if it were shown that a 20-dollar ore could be mined to a profit.

Several English companies are now entering on active mining operations in Gilpin County, and it is to be hoped they will inaugurate a new system. This they are the more likely to do, as the properties they have purchased are not hampered with old stamp mills.

III. CONDITION OF THE MOON'S SURFACE.

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If the study of our earth's crust—or the science of Geology—is capable of throwing some degree of light on the past condition of other members of the solar system, the study of those other orbs seems capable of at least suggesting useful ideas concerning the past condition of our earth. There are members of the solar system respecting which it may reasonably be inferred that they are in an earlier stage of their existence than the earth. Jupiter and Saturn, for instance, would seem—so far as observation has extended—to be still in a condition of intense heat, and still the seat of forces such as were once probably at work within our earth. We see these planets enwrapped, to all appearance, within a double or triple coating of clouds, and we are compelled to infer, from the behaviour of these clouds, that they are generated by forces belonging to the orb which they envelope; we have, also, every reason which the nature of the case can afford to suppose that our own earth was once similarly cloud-enveloped. We can scarcely imagine that in the long-past ages, when the igneous rocks were in the primary stages of their existence, the air was not loaded heavily with clouds. We may, then, regard Jupiter and Saturn as to some degree indicating the state of our own earth at a long-past epoch of her existence. On the other hand, it has been held, and not without some degree of evidence in favour of the theory, that in our moon we have a picture of our earth as she will be at some far-distant future date, when her period of rotation has been forced into accordance with the period of the moon's revolution round the earth, when the internal heat of the earth's globe has been radiated almost wholly away into space, and when her oceans and atmosphere have disappeared through the action of the same circumstances (whatever they may be) which have caused the moon to be air-less and ocean-less. But whether we take this view of our earth's future, or whether we consider that her state has been from the beginning very different from that of the moon, it nevertheless remains probable that we see in our moon a globe which has passed through a much greater proportion of its history (so to speak) than our earth; and accordingly the study of the moon's condition seems capable of giving some degree of information as to the future (possibly also as to the past) of our earth.

I wish, in the present paper, to consider the moon's condition from a somewhat different point of view than has commonly been adopted. It appears to me that the study fo the moon's surface with the telescope, and the consideration of the various phenomena which give evidence on the question whether air or water exist anywhere upon or within her, have not as yet led to any satisfactory inferences as to her past history. We see the traces of tremendous sublunarian disturbances (using the word "sublunarian," here and elsewhere, to correspond to the word "subterranean" used with reference to the earth), and we find some features of resemblance between the effects of such disturbances and those produced by the subterranean forces of our earth; but we find also as marked signs of distinction between the features of the lunar and terrestrial crusts. Again, comparing the evidences of a lunar atmosphere with those which we should expect if an atmosphere like our own surrounded the moon, we are able to decide, with some degree of confidence, that the moon has either no atmosphere or one of very limited extent. But there our knowledge comes to an end; nor does it seem likely that, by any contrivances man can devise, the further questions which suggest themselves respecting the moon's condition can be answered by means of observation.

But there are certain considerations respecting the moon's past history which seem to me likely, if duly weighed, to throw some light on the difficult problems presented by the moon.

In the first place, it is to be noted that the peculiar relation between the moon's rotation and revolution possesses a meaning which has not hitherto, so far as I know, been attended to. We know that *now* there is an absolutely perfect agreement between the moon's rotation and revolution, in this respect—that her mean period of rotation on her axis is exactly equal to her mean period of revolution. (Here either sidereal rotation and revolution or synodical rotation and revolution may be understood, so long as both revolution and rotation are understood to be of the same kind). I say "mean period of rotation," for although as a matter of fact it is only the revolution which is subject to any considerable variation, the rotation also is not perfectly uniform. We know, furthermore, that if there had been, long ago, a *near* agreement between the mean rotation and revolution, the present exact agreement would have resulted, through the effects of the mutual attractions of the earth and moon. But, so far as I know, astronomers have not yet carefully considered the question whether that close agreement existed from the beginning, or was the result of other forms of action than are at present at work. If it existed from the beginning, that is from the moon's first existence as a body independent of the earth, it is a matter requiring to be explained, as it implies a peculiar relation between the moon and earth before the present state of things existed. If, on the contrary, it has been brought about by the amount of action which is now gradually reducing the earth's rotation period, we have first of all to consider that an enormous period of time has been required to bring the moon to her present condition in this respect, and, moreover, that either an ocean existed on her surface or that her crust was once in so plastic a condition as to be traversed by a tidal wave resembling, in some respects, the tidal wave in our own ocean. This, at any rate, is what we must believe if we suppose, first, that the main cause of the lengthening of the terrestrial day is the action of the tidal wave as a sort of brake on the earth's rotating globe, and, secondly, that a similar cause produced the lengthening of the moon's day to its present enormous duration. It may be, as we shall presently see, that other causes have to be taken into account in the moon's case.

Now we are thus, either way, brought to a consideration of that distant epoch when—according to the nebular theory, or any admissible modification thereof—the moon was as yet non-existent as an orb distinct from the earth. We must suppose, on one theory, that the moon was at that time enveloped in the nebulous rotating spheroid out of which the earth was to be formed, she herself (the moon) being a nebulous sub-spheroid within the other, and so far coerced by the motion of the other that her longer axis partook in its motion of rotation. Unquestionably in that case, as the terrestrial spheroid contracted and left the other as a separate body, this other, or lunar spheroid, would exhibit the kind of rotation which the moon actually possesses. On the other theory, we should be led to suppose that primarily the lunar spheroid rotated independently of its revolution; but that the earth's attraction acting on the outer shells, after they had become first fluid and then (probably) viscous, produced waves travelling in the same direction as the rotation, but with a continual brake-action, tending slowly to reduce the rotation until it had its present value, when dynamical equilibrium would be secured.

But, as I have said, in either case we must trace back the moon's history to an epoch when she was in a state of intense heat. And it seems to me that we are thus led to notice that the development of the present state of things in the moon must have taken place during an era in the history of the solar system differing essentially from that which prevailed during the later and better-known geological eras of our own earth. Our moon was *shaped*, so to speak, when the solar system itself was young, when the sun may have given out a much greater degree of heat than at present, when Saturn and Jupiter were brilliant suns, when even our earth and her fellow minor planets within the zone of asteroids were probably in a sun-like condition. Putting aside all hypothesis, it nevertheless remains clear that, to understand the moon's present condition, we must form some estimate of the probable condition of the solar system in distant eras of its existence; for it was in such eras, and not in an era like the present, that she was modelled to her present figure.

It appears to me that we are thus, to some extent, freed from a consideration which has proved a difficulty to many who have theorised respecting the moon. It has been said that the evidence of volcanic action implies the existence, at least when that action was in progress, of an atmosphere capable of supporting combustion,—in other words, an atmosphere containing oxygen, for other forms of combustion than those in which oxygen plays a part may here be dismissed from consideration. But the fiery heat of the moon's substance may have been maintained (in the distant eras to which we are now referring the formation of her crust) without combustion. Taking the nebular hypothesis as it is commonly presented, the moon's globe may have remained amid the intensely hot nebulous spheroid (which was one day to contract, and so form the globe of the earth) until the nebula left it to cool thenceforth rapidly to its present state. Whatever objections suggest themselves to such a view are precisely the objections which oppose themselves to the simple nebular hypothesis, and may be disposed of by those who accept that hypothesis. But better, to my view, it may be reasoned, that the processes of contraction and of the gathering in of matter from without, which maintained the heat of the nebulous masses, operated to produce all the processes of disturbance which brought the moon to her present condition, and that thus there was not necessarily any combustion whatever. Indeed, in any case, combustion can only have commenced when the heat had been so far reduced that any oxygen existing in the lunar spheroid would enter into chemical combination with various components of the moon's glowing substance. If there were no oxygen (an unlikely supposition, however), the moon's heat would nevertheless have been maintained so long as meteoric impact on the one hand, and contraction of the moon's substance on the other, continued to supply the requisite mechanical sources of heat-generation. In this case there would not necessarily have been any gaseous or vapourous matter, other than the matter retained in the gaseous condition by intensity of heat, and becoming first liquid and afterwards solid, so soon as the heat was sufficiently reduced.

It must here be considered how far we have reason to believe that the heat of the various members of the solar system—including the moon and other secondary bodies—was originally produced, and thereafter maintained, by collisions; because it is clear that, as regards the surface contour of these bodies, much would depend on this circumstance. There would be a considerable difference between the condition of a body which was maintained at a high temperature for a long period, and eventually cooled, but slowly, under a continual downfall of matter, and that of a body whose heat was maintained by a process of gradual contraction. It is true that in the case of a globe like the earth, whose surface was eventually modelled and re-modelled by processes of a totally different kind, by deposition and denudation, by wind and rain, river-action and the beating of seas, the signs of the original processes of cooling would to a great extent disappear; but if, as we are supposing in the case of the moon, there was neither water nor air (at least in sufficient quantity to produce any effect corresponding to those produced by air and water on the earth), the principal features of the surface would depend largely on the conditions under which the process of cooling began and proceeded.

Now here I must recall to the attention of the reader the reasoning which I have made use of in my "Other Worlds than Ours," to show that, in all probability, our solar system owed its origin rather to the gathering of matter together from outer space than to the contraction of a rotating nebulous mass. It is there shown, and I think that the consideration is one which should have weight in such an inquiry, that there is nothing in the nebular hypothesis of Laplace to account in any degree for the peculiarities of detail presented by the solar system. That theory explains the revolution of the members of the solar system in the same direction, their rotation in the same direction, the approach to circularity of the orbits, and their near coincidence with the mean plane of the system; but it leaves altogether unexplained the different dimensions of the primary members of the solar system, the apparent absence of law and order in their axial tilt, and the inclination of the orbits of their satellite families. In particular, the remarkable difference which exists between the outer family of planets,—the giant orbs, Jupiter, Saturn, Uranus, and Neptune,—and the inner family of small planets,—Mars, the Earth, Venus, and Mercury,—is left wholly unexplained. Nor can one recognise in the nebular hypothesis any reason whatever for the comparative exuberance of orb-forming activity in the outer family, and particularly in the two planets lying next to the zone of asteroids, and the poverty of material which is exhibited within the minor family of planets. All these circumstances appear to be explained satisfactorily when we regard the solar system as formed by the gathering in from outer space of materials once widely scattered. We can see that in the neighbourhood of the great primary centre there would be indeed a great abundance of gathered and gathering matter, but that, owing to the enormous velocities in that neighbourhood, subordinate centres of attraction would there form slowly, and acquire but moderate dimensions. Outside a certain distance there would be less matter, but a far greater freedom of aggregation; there we should find the giant secondary centres, and we should expect the chief of these to lie inwards, as Jupiter and Saturn, while beyond would be orbs vast indeed, but far inferior to these planets. And we can readily see that the border region between the family of minor planets and the family of major planets would be one where the formation of a planet would be rendered unlikely; here, therefore, we should look for the existence of a zone of small bodies like the asteroids. I touch on these points to show the kind of evidence (elsewhere given at length) on which I have based my opinion that the solar system had its birth, and long maintained its fires, under the impact and collisions of bodies gathered in from outer space.

According to this view, the moon, formed at a comparatively distant epoch in the history of the solar system, would have not merely had its heat originally generated for the most part by meteoric impact, but while still plastic would have been exposed to meteoric downfalls, compared with which all that we know, in the present day, of meteor-showers, aërolitic masses, and so on, must be regarded as altogether insignificant. It would be to such downfall mainly that the maintenance of the moon's heat would at that time be due, though, as we shall presently see, processes of contraction must have not only supplemented this source of heat-supply, but must have continued to maintain the moon's heat long after the meteoric source of heat had become comparatively ineffective.

Now, I would notice in passing that here we *may* find an explanation of the agreement between the moon's rotation period and her period of revolution. It is clear that under the continuous downfall of meteoric matter in that distant era, the moon must have been in a process of actual growth. She is indeed growing *now* from the same cause; and so is the earth: but such growth must be regarded as infinitesimally small. In the earlier periods of the moon's history, on the contrary, the moon's growth must have progressed at a comparatively rapid rate. Now this influx of matter must have resulted in a gradual reduction of the moon's rate of rotation, if (as we must suppose) the moon gathered matter merely by chance collisions. In the case of a globe gathering in matter by its own attractive power as the sun does, for instance, the arriving matter may (owing to the manner in which the process is effected) serve to maintain and even to increase the rate of rotation; but in the case of a subordinate body like the moon we must suppose that all effects acting on the rotation would be about equally balanced, and that the sole really effective result would be the increase of the moon's bulk, and the consequent diminution of her rotation rate. Now, if this process continued until the rotation rate had nearly reached its present value, the earth's attraction would suffice not merely to bring the rate of rotation precisely to its present value, but to prevent its changing (by the continuance of the process) to a smaller value. It may be added that the increase in the moon's rate of revolution, as she herself and the earth both grew under meteoric downfall towards their present dimensions, would operate in a similar way,—it would tend to bring the moon's rate of revolution and her rate of rotation towards that agreement which at present exists.

If we attempt to picture the condition of the moon in that era of her history when first the process of downfall became so far reduced in activity as to permit of her cooling down, we shall be tempted, I believe, to consider that some of the more remarkable features of her globe had their origin in that period. It may seem, indeed, at a first view, too wild and fanciful an idea to suggest that the multitudinous craters on the moon, and especially the smaller craters revealed in countless numbers when telescopes of high power are employed, have been caused by the plash of meteoric rain,—and I should certainly not care to maintain that as the true theory of their origin; yet it must be remembered that no plausible theory has yet been urged respecting this remarkable feature of the moon's surface. It is impossible to recognise a real resemblance between any terrestrial feature and the crateriferous surface of the moon. As blowholes, so many openings cannot at any time have been necessary, whatever opinion we may form as to the condition of the moon's interior and its reaction upon the crust. Moreover, it should be remembered that our leading seismologists regard water as absolutely essential to the production of volcanic disturbance (the only form of disturbance which on our earth leads to the formation of cup-shaped openings). If we consider the explanation advanced by Hooke, that these numerous craters were produced in the same way that small cup-shaped depressions are formed when thick calcareous solutions are boiled and left to cool, we see that it is inadequate to account for lunar craters, the least of which (those to which Mr. Birt has given the name of craterlets) are at least half a mile in diameter. The rings obtained by Hooke were formed by the breaking of surface bubbles or blisters,* and it is impossible for such bubbles to be formed on the scale of the lunar craters. Now so far as the smaller craters are concerned, there is nothing incredible in the supposition that they were due to meteoric rain falling when the moon was in a plastic condition. Indeed, it is somewhat remarkable how strikingly certain parts of the moon resemble a surface which has been rained upon while sufficiently plastic to receive the impressions, but not too soft to retain them. Nor is it any valid objection to this supposition, that the rings left by meteoric downfall would only be circular when the falling matter chanced to strike the moon's surface squarely; for it is far more probable that even when the surface was struck very obliquely and the opening first formed by the meteoric mass or cloud of bodies was therefore markedly elliptic, the plastic surface would close in round the place of impact until the impression actually formed had assumed a nearly circular shape.

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* "Presently ceasing to boil," he says of alabaster, "the whole surface will appear covered all over with small pits, exactly shaped like those of the moon." "The earthy part of the moon has been undermined," he proceeds, "or heaved up by eruptions of vapour, and thrown into the same kind of figured holes as the powder of alabaster."

Before passing from this part of my subject, I would invite attention to the aspect of the half moon as presented in the photograph illustrating this paper (see Frontispiece).* It will be seen that the multitudinous craters near the top of the picture (the southern part of the moon†) are strongly suggestive of the kind of process I have referred to, and that, in fact, if one judged solely by appearances, one would be disposed to adopt somewhat confidently the theory that the moon had had her present surface craters chiefly formed by meteoric downfalls during the period of her existence when she was plastic to impressions from without. I am, however, sensible that the great craters under close telescopic scrutiny by no means correspond in appearance to what we should expect if *they* were formed by the downfall of great masses from without. The regular, and we may almost say battlemented, aspect of some of these craters, the level floor, and the central peaks so commonly recognised, seem altogether different from what we should expect if a great mass fell from outer space upon the moon's surface. It is indeed just possible that under the tremendous heat generated by the downfall a vast circular region of the moon's surface would be rendered liquid, and that in rapidly solidifying while still traversed by the ring-waves resulting from the downfall, something like the present condition would result. Or we might suppose that the region liquefied through the effects of the shock was very much larger than the meteoric mass; and that while a wave of disturbance travelled outwards from the place of impact to be solidified (owing to rapid radiation of heat) even as it travelled, a portion of the liquid interior of the moon forced its way through the opening formed by the falling mass. But such ideas as these require to be supported by much stronger evidence than we possess before they can be regarded as acceptable. I would remark, however, that nothing hitherto advanced has explained at all satisfactorily the structure of the great crateriform mountain ranges on the moon. The theory that there were once great lakes seems open to difficulties at least as grave as the one I have just considered, and to this further objection, that it affords no explanation of the circular shape of these lunar regions. On the other hand, Sir John Herschel's account of the appearance of these craters is not supported by any reasoning based on our knowledge of the actual circumstances under which volcanic action proceeds in the case of our own earth. "The generality of the lunar mountains," he says, "present a striking uniformity and singularity of aspect. They are wonderfully numerous, occupying by far the larger portion of the surface, and almost universally of an exact circular or cup-shaped form, foreshortened, however, into ellipses towards the limb; but the larger have for the most part flat bottoms within, from which rises centrally a small, steep, conical hill. They offer, in short, in its highest perfection the true volcanic character, as it may be seen in the crater of Vesuvius; and, in some of the principal ones, decisive marks of volcanic stratification, arising from successive deposits of ejected matter, may be clearly traced with powerful telescopes. What is, moreover, extremely singular in the geology of the moon is, that although nothing having the character of seas can be traced (for the dusty spots which are commonly called seas, when closely examined, present appearances incompatible with the supposition of deep water), yet there are large regions perfectly level, and apparently of a decided alluvial character?"

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* This photograph is interesting as the work of the Great Melbourne reflector. It was taken directly of its present size, and in this respect differs from all others of the same size, since, hitherto the negatives taken have been small.

† Owing to the fact that this photograph has been taken with a Newtonian reflector, we have not the same kind of inversion as in the case of photographs taken with refractors. In the latter case all that is necessary to cause the picture to represent the moon as we see her, is simply to hold the picture upside down; but the photograph illustrating this paper will only resemble the half moon as she actually appears (at the time of first quarter, the epoch of the photograph) by holding the picture inverted before a looking-glass. The picture would also show rightly if inverted and then looked at from behind, supposing the method of mounting such that the picture can be seen from behind when held up between the eye and the light. At present I do not know whether this will be the case or not.

It is obvious that in this description we have, besides those features of volcanic action which might perhaps be expected on the moon, a reference to features essentially terrestrial. Alluvial deposits can have no existence, for example, save where there are rivers and seas, as well as an atmosphere within which clouds may form, whence rain may be poured upon the surface of wide land regions. It is not going too far to say that we have the clearest evidence to show that in the moon none of these conditions are fulfilled. Whether in former ages lunar oceans and seas and a lunar atmosphere have existed, may be a doubtful point; but it is certain that all the evidence we have is negative, save only those extremely doubtful signs of glacier action recognised by Prof. Frankland. I venture to quote from Guillemin's "Heavens" a statement of Frankland's views, in order that the reader may see on how slender a foundation hypotheses far more startling than the theory I have suggested have been based by a careful reasoner and able physicist. "Prof. Frankland believes," says the account, "and his belief rests on a special study of the lunar surface, that our satellite has, like its primary, also passed through a glacial epoch, and that several, at least, of the *valleys*, *rills*, and *streaks* of the lunar surface are not improbably due to former glacial action. Notwithstanding the excellent definition of modern telescopes, it could not be expected that other than the most gigantic of the characteristic details of an ancient glacier-bed would be rendered visible. What, then, may we expect to see? Under favourable circumstances the terminal moraine of a glacier attains enormous dimensions; and consequently of all the marks of a glacier valley this would be the one most likely to be first perceived. Two such terminal moraines, one of them a double one, have appeared to observers to be traceable upon the moon's surface. The first is situated near the termination of the remarkable streak which commences near the base of Tycho, and passing under the south-eastern wall of Bullialdus, into the ring of which it appears to cut, is gradually lost after passing Lubiniezky. Exactly opposite this last, and extending nearly across the streak in question, are two ridges forming the arcs of circles whose centres are not coincident, and whose external curvature is towards the north. Beyond the second ridge a talus slopes gradually down northwards to the general level of the lunar surface, the whole presenting an appearance reminding the observer of the concentric moraines of the Rhône glacier. These ridges are visible for the whole period during which that portion of the moon's surface is illuminated; but it is only about the third day after the first quarter, and at the corresponding phase of the waning moon, when the sun's rays, falling nearly horizontally, throw the details of this part of the surface into strong relief; and these appearances suggest this explanation of them. The other ridge answering to a terminal moraine, occurs at the northern extremity of that magnificent valley which runs past the eastern edge of Rheita."

Here are two lunar features of extreme delicacy, and certainly not incapable of being otherwise explained, referred by Frankland to glacier action. It need hardly be said that glacial action implies the existence of water and an atmosphere on the moon,—and not only so, but there must have been extensive oceans and an atmosphere nearly equal in density to that of our own earth, if the appearances commented upon by Frankland were due to glacial action. It is admitted by Frankland, of course, that there is now no evidence whatever of the presence of water, "but, on the contrary, all selenographical observations tend to prove its absence. Nevertheless," proceeds the account from which I have already quoted, "the idea of former aqueous agency in the moon *has received almost universal acceptation*" (the italics are mine). "It was entertained by Gruithuisen and others. But, if water at one time existed on the surface of the moon, whither has it disappeared? If we assume, in accordance with the nebular hypothesis, that the portions of matter composing respectively the earth and the moon once possessed an equally elevated temperature, it almost necessarily follows that the moon, owing to the comparative smallness of her mass, would cool more rapidly than the earth; for whilst the volume of the moon is only about 1-49th (and its mass, it might be added, only about 1-81st part), its surface is nearly 1-13th that of the earth. This cooling of the mass of the moon must, in accordance with all analogy, have been attended with contraction, which can scarcely be conceived as occurring without the development of a cavernous structure in the interior. Much of this cavernous structure would doubtless communicate, by means of fissures, with the surface; and thus there would be provided an internal receptacle for the ocean, from the depths of which even the burning sun of the long lunar day would be totally unable to dislodge more than traces of its vapour. Assuming the solid mass of the moon to contract on cooling at the same rate as granite, its refrigeration though only 180° F. would create cellular space equal to nearly 14½ millions of cubic miles, which would be more than sufficient to engulf the whole of the lunar oceans, supposing them to bear the same proportion to the mass of the moon as our own oceans bear to that of the earth."

The great objection to this view of the moon's past history consists in the difficulty of accounting for the lunar atmosphere. It must be remembered that owing to the smallness of the moon's mass, an atmosphere composed in the same way as ours would have a much greater depth compared with its density at the mean level of the moon's surface than our atmosphere possesses compared with its pressure at the sea-level. If there were exactly the same quantity of air above each square mile of the moon's surface as there is above each square mile of the earth's surface, the lunar air would not only extend to a much greater height than ours, but would be much less dense at the moon's surface. The atmospheric pressure would in that case be about 1-6th that at our sea-level, and instead of the lower half of such an atmosphere (that is, the lower half in actual quantity of air) lying within a distance of about 3½ miles from the mean surface, as in the case of our earth, it would extend to a distance of about 22 miles from the surface. Now this reasoning applies with increased force to the case of an atmosphere contained within the cavernous interior of the moon; for there the pressure due to the attraction of the moon's mass would be reduced. It is very difficult to conceive that under such circumstances room would not only exist for lunar oceans, but for a lunar atmosphere occupying, one must suppose, a far greater amount of space even before their withdrawal into these lunar caverns, and partially freed from pressure so soon as such withdrawal had taken place. That the atmosphere should be withdrawn so completely that no trace of its existence could be recognised does certainly appear very difficult to believe, to say the least.

Nevertheless, it is not to be forgotten that so far as terrestrial experience is concerned water is absolutely essential to the occurrence of volcanic action. If we are to extend terrestrial analogies to the case of our moon, notwithstanding the signs that the conditions prevailing in her case have been very different from those existing in the case of our earth, we are bound to recognise at least the possibility that water once existed on the moon. Moreover, it must be admitted that Professor Frankland's theory seems to accord far better with lunar facts than any of the others which have been advanced to account for the disappearance of all traces of water or air. The theory that oceans and an atmosphere have been drawn to the farther side of the moon cannot be entertained when due account is taken of the range of the lunar librations. Sir J. Herschel, indeed, once gave countenance to that somewhat *bizarre* theory; but he admitted in a letter addressed to myself, that the objection I had based on the circumstances of libration was sufficient to dispose of the theory. The hypothesis that a comet had whisked away the lunar oceans and atmosphere does not need serious refutation; and it is difficult to see how the theory that lunar seas and lunar air have been solidified by intense cold can be maintained in presence of the fact that experiments made with the Rosse mirror indicate great intensity of heat in the substance of those parts of the moon which have been exposed to the full heat of the sun during the long lunar day.

If there ever existed a lunar atmosphere and lunar seas, then Prof. Frankland's theory seems the only available means of accounting for their disappearance. Accordingly we must recognise the extreme interest and importance of telescopic researches directed to the inquiry, whether any features of the moon's surface indicate the action of processes of *weathering*, whether the beds of lunar rivers can anywhere be traced, whether the shores of lunar seas can be recognised by any of those features which exist round the coast-lines of our own shores.

One circumstance may be remarked in passing. If the multitudinous lunar craters were formed before the withdrawal of lunar water and air into the moon's interior, it is somewhat remarkable that the only terrestrial features which can be in any way compared with them should be found in regions of the earth which geologists regard as among those which certainly have not been exposed to denudation by the action of water. Thus Sir John Herschel, speaking of the extinct volcanoes of the Puy de Dôme, remarks that here the observer sees "a magnificent series of volcanic cones, fields of ashes, streams of lava, and basaltic terraces or platforms, proving the volcanic action to have been continued for countless ages before the present surface of the earth was formed; here can be seen a configuration of surface quite resembling what telescopes show in the most volcanic districts of the moon; for half the moon's face is covered with unmistakable craters of extinct volcanoes." But Lyell, speaking of the same volcanic chains, describes them as regions "where the eruption of volcanic matter has taken place in the open air, and where the surface has never since been subjected to great aqueous denudation." If all the craters on the moon belonged to one epoch, or even to one era, we might regard them as produced during the withdrawal of the lunar oceans within the still heated substance of our satellite. But it is manifest that the processes which brought the moon's surface to its present condition must have occupied many ages, during which the craters formed earliest would be exposed to the effects of denudation, and to other processes of which no traces can be recognised. It is not likely, however, that the withdrawal of the lunar oceans into the moon's cavernous interior can have taken place suddenly; up to a certain epoch the entry of the waters within the moon's mass would be impossible, owing to the intense heat, which, by maintaining the plasticity of the moon's substance, would prevent the formation of cavities and fissures, while any water brought into contact with the heated interior would at once be vaporised, and driven away. But when once a condition was attained which rendered the formation of cavities possible, the contraction of the moon's substance would lead to the gradual increase of such cavities, and so, as time proceeded, room would be found for all the lunar oceans.

We are next led to the inquiry whether the contraction of the moon's substance may not have played the most important part of all, in producing those phenomena of disturbance which are presented by the moon's surface. Quite recently the eminent seismologist Mallet has propounded a theory of terrestrial volcanic energy, which not only appears to account—far more satisfactorily than any hitherto adopted—for the phenomena presented by the earth's crust, but suggests considerations which may be applied to the case of the moon, and in fact are so applied by Mallet himself. It behoves us to inquire very carefully into the bearing of this theory upon the subject of lunar seismology, and therefore to consider attentively the points in which the theory differs from those hitherto adopted.

Mallet dismisses first the chemical theory of volcanic energy, because all known facts tend to show that the chemical energies of the materials of our globe were almost wholly exhausted prior to the consolidation of its surface. This may be regarded as equally applicable to the case of the moon. It is difficult to see how the surface of the moon can have become consolidated while any considerable portion of the chemical activity of her materials remained unexhausted.

"The mechanical theory," proceeds Mallet, "which finds in a nucleus still in a state of liquid fusion a store of heat and of lava, &c., is only tenable on the admission of a very thin solid crust; and even through a crust but 30 miles thick, it is difficult to see how surface-water is to gain access to the fused nucleus; *yet without water there can be no volcano.* More recent investigation on the part of mathematicians has been supposed to prove that the earth's crust is not thin." He proceeds to show that, without attaching any great weight to these mathematical calculations, there are other grounds for believing that the solid crust of the earth is of great thickness, and that "although there is evidence of a nucleus much hotter than the crust, there is no certainty that any part of it remains liquid; but if so, it is in any case too deep to render it conceivable that surface-water should make its way down to it. The results of geological speculation and of physico-mathematical reasoning thus oppose each other; so that some source of volcanic heat closer to the surface remains to be sought. The hypothesis to supply this, proposed by Hopkins and adopted by some, viz., of isolated subterranean lakes of liquid matter, in fusion at no great depth from the surface, remaining fused for ages, surrounded by colder and solid rock, and with (by hypothesis) access of surface-water, seems feeble and unsustainable."

Now in some respects this reasoning is not applicable to the moon, at least so far as real evidence is concerned; though it is to be noticed that, if a case is made out for any cause of volcanic action on the earth, we are led by analogy to extend the reasoning (or at least its result) to the case of the moon. But it may be remarked that the solidification of the moon's crust must have proceeded at a more rapid rate than that of the earth's, while the proportion of its thickness to the volume of the fused nucleus would necessarily be greater for the same thickness of the crust. The question of the access of water brings us to the difficulty already considered,—the inquiry, namely, whether oceans originally existed on the moon. For the moment, however, we forbear from considering whether Mallet's reasoning must necessarily be regarded as inapplicable to the moon if it should be admitted that there never were any lunar oceans.

We come now to Mallet's solution of the problem of terrestrial volcanic energy.

We have been so long in the habit of regarding volcanoes and earthquakes as evidences of the earth's subterranean forces,—as due, in fact (to use Humboldt's expression), to the reaction of the earth's interior upon its crust,—that the idea presents itself at first sight as somewhat startling, that all volcanic and seismic phenomena, as well as the formation of mountain ranges, have been due to a set of cosmical forces called into play by the *contraction* of our globe. According to the new theory, it is not the pressure of matter under the crust outwards, but the pressure of the earth's crust inwards, which produces volcanic energy. Nor is this merely substituting an action for reaction, or *vice versa.* According to former views, it was the inability of the crust to resist pressure from within which led to volcanic explosions, or which produced earthquake throes where the safety-valve provided by volcanoes was not supplied. The new theory teaches, in fact, that it is a deficiency of internal resistance, and not an excess, which causes these disturbances of the crust. "The contraction of our globe," says Mallet,* "has been met, from the period of its fluidity to its present state,—first, by deformation of the spheroid, forming generally the ocean-basins and the land; afterwards by the foldings over and elevations of the thickened crust into mountain-ranges, &c.; and, lastly, by the mechanism which gives rise to volcanic actions. The theory of mountain elevation proposed by C. Prévost was the only true one,—that which ascribes this to tangential pressures propagated through a solid crust of sufficient thickness to transmit them, these pressures being produced by the relative rate of contraction of the nucleus and of the crust; the former being at a higher temperature, and having a higher coefficient of contraction for equal loss of heat, tends to shrink away from beneath the crust, leaving the latter partially unsupported. This, which during a much more rapid rate of cooling from higher temperature of the whole globe, and from a thinner crust, gave rise in former epochs to mountain-elevation, in the present state of things gives rise to volcanic heat." By the application of a theorem of Lagrange, Mr. Mallet proves that the earth's solid crust, however great may be its thickness, "and even if of materials far more cohesive and rigid than those of which we must suppose it to consist, must, if even to a very small extent left unsupported by the shrinking away of the nucleus, crush up in places by its own gravity, and by the attraction of the nucleus. This is actually going on; and in this partial crushing," at places or depths dependent on the material and on conditions which Mr. Mallet points out, he discerns "the true cause of volcanic heat.† As the solid crust sinks together to follow down after the shrinking nucleus, the *work* expended in mutual crushing and dislocation of its parts is *transformed into heat*, by which, at the places where the crushing sufficiently takes place, the material of the rock so crushed and of that adjacent to it are heated even to fusion. The access of water to such points determines volcanic eruption. Volcanic heat, therefore, is one result of the secular cooling of a terraqueous globe subject to gravitation, and needs no strange or gratuitous hypothesis as to its origin."

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* I quote throughout from an abstract of Mallet's paper in the "Philosophical Magazine" for December, 1872. The words are probably, for the most part, Mallet's own; but I have not the original paper by me for reference. I believe, however, that the abstract is from his own pen.

† "In order to test the validity of his theory by contact with known facts" (says the "Philosophical Magazine"), "Mr. Mallet gives in detail two im-

It is readily seen how important a bearing these conclusions have upon the question of the moon's condition. So far, at any rate, as the processes of contraction and the consequent crushing and dislocation of the crust are concerned, we see at once that in the case of the moon these processes would take place far more actively than in the earth's case. For the cooling of the moon must have taken place far more rapidly, and the excess of the contraction of the nucleus over that of the crust must have been considerably greater. Moreover, although the force of gravity is much less on the moon than on our earth, and therefore the heat developed by any process of contraction correspondingly reduced, yet, on the one hand, this would probably be more than compensated by the greater activity of the lunar contraction (*i.e.*, by the more rapid reduction of the moon's heat), and on the other, the resistance to be encountered in the formation of elevations by this process would be reduced

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portant series of experiments completed by him :—the one on the actual amount of heat capable of being developed by the crushing of sixteen different species of rocks, chosen so as to be representative of the whole series of known rock-formations from oolites down to the hardest crystalline rocks; the other, on the coefficients of total contraction between fusion and solidification, at existing mean temperature of the atmosphere, of basic and acid slags analogous to melted rocks. The latter experiments were conducted on a very large scale; and the author points out the great errors of preceding experimenters, Bischoff and others, as to these coefficients. By the aid of these experimental data, he is enabled to test the theory produced when compared with such facts as we possess as to the rate of present cooling of our globe, and the total annual amount of volcanic action taking place upon its surface and within its crust. He shows, by estimates which allow an ample margin to the best data we possess as to the total annual vulcanicity, of all sorts, of our globe at present, that less than one-fourth of the total heat at present annually lost by our globe is upon his theory sufficient to account for it; so that the secular cooling, small as it is, now going on, is a sufficient *primum mobile*, leaving the greater portion still to be dissipated by radiation. The author then brings his views into contact with known facts of vulcanology and seismology, showing their accordance. He also shows that to the heat developed by partial tangential thrusts within the solid crust are due those perturbations of hypogeal increment of temperature which Hopkins has shown cannot be referred to a cooling nucleus and to differences of conductivity alone."

precisely in the same proportion that gravity is less at the moon's surface. It is important to notice that, as Mr. Mallet himself points out, his view of the origin of volcanic heat "is independent of any particular thickness being assigned to the earth's solid crust, or to whether there is at present a liquid fused nucleus,—all that is necessary being a *hotter* nucleus than crust, so that the rate of contraction is greater for the former than for the latter." Moreover, "as the play of tangential pressures has elevated the mountain-chains in past epochs, the nature of the forces employed sets a limit" to the possible height of mountains on our globe. This brings Mr. Mallet's views into connection with "vulcanicity produced in like manner in other planets, or in our own satellite, and supplies an adequate solution of the singular, and so far unexplained, fact, that the elevations upon our moon's surface and the evidences of former volcanic activity are upon a scale so vast when compared with those upon our globe."

All that seems wanted to make the explanation of the general condition of the moon's surface complete, according to this theory, is the presence of water in former ages, over a large extent of the moon's surface,—*unless* we combine with the theory of contraction the further supposition that the downfall of large masses on the moon produced that local fusion which is necessary to account for the crateriform surface-contour. It is impossible to contemplate the great mountain-ranges of the moon (as, for instance, the Apennines under favourable circumstances of illumination), without seeing that Mallet's theory accords perfectly with their peculiar corrugated aspect (the same aspect, doubtless, which terrestrial mountain-ranges would exhibit if they could be viewed as a whole from any suitable station). Again, the aspect of the regions surrounding the great lunar craters—and especially the well-studied crater Copernicus—accords closely, when sufficient telescopic power is employed, with the theory that there has been a general contraction of the outer crust of the moon, resulting in foldings and cross-foldings, wrinkles, corrugations, and nodules. But the multiplicity of smaller craters does not seem to be explained at all satisfactorily; while the present absence of water, as well as the want of any positive or direct evidence that water ever existed upon the moon, compels us to regard even the general condition of the moon's surface as a problem which has still to be explained. If, however, it be admitted that the processes of contraction proceeded with sufficient activity to produce fusion in the central part of a great region of contracting crust, and that the heat under the crust sufficed for the vaporisation of a considerable portion of the underlying parts of the moon's substance, we might find an explanation of the great craters like Copernicus, as caused by true volcanic action. The masses of vapour which, according to that view, sought an outlet at craters like Copernicus must have been enormous however. Almost immediately after their escape they would be liquefied, and flow down outside the raised mouth of the crater. According to this view we should see, in the floor of the crater, the surface of what had formerly been the glowing nucleus of the moon: the masses near the centre of the floor (in so many cases) might be regarded as, in some instances, the *débris* left after the great outburst, and in others as the signs of a fresh outburst proceeding from a yet lower level; while the glistening matter which lies all round many of the monster craters would be regarded as the matter which had been poured out during the outburst.

We need not discuss in this connection the minor phenomena of the moon's surface. It seems evident that the *rilles*, and all forms of *faults* observable on the moon's surface, might be expected to result from such processes of contraction as Mallet's theory deals with.

It is, in fact, the striking features of the moon's disc—those which are seen when she is examined with comparatively low telescopic powers—which seem to tax most severely every theory which has yet been presented. The clustering craters, which were compared by Galileo to "eyes upon the peacock's tail," remain unaccounted for hitherto; and so do the great dark regions called seas. Mallet's theory explains, perhaps, the varieties of level observed in the moon's surface-contour, but the varieties of tint and colour remain seemingly inexplicable.

There is one feature of the lunar globe which presents itself to us under a wholly changed aspect if we adopt Mallet's theory. I refer to the radiations from certain great craters, and especially those from Tycho, Copernicus, Kepler, and Aristarchus. The reader is doubtless aware that an attempt has been made to explain these radiations by comparing them to the fissures produced when hollow globes are burst by pressure from within. It is in this way that Mr. Nasmyth accounts for these striking features of the moon's disc. But it has been objected that if such fissures were formed and filled up by matter extruded from the interior of the satellite, it could not but happen that along some portions of the length of each fissure the original contour of the surface would not be restored,—either an excess of matter being forced up through the opening or a part of the opening left unfilled,—and that the resulting inequalities could not fail to be rendered discernible under oblique illumination. According to any theory which accounted for these features as due to internal forces acting outwards, it was exceedingly difficult to interpret the fact that along the whole length of these rays there can be observed a peculiar difference of brightness under direct illumination, while, nevertheless, such features of the surface as craters, mountain-ranges, plains, and so on, extend unbroken over the rays. I do not know that the theory of contraction serves to meet the difficulty completely; in fact, the difference of tint in the rays and the circumstance that the rays can only be well seen under full illumination appear to me to be among the most perplexing of the many perplexing phenomena presented by the moon's surface. But so far as the mere formation of radiations of enormous length is concerned, it seems to me that we have a far more promising interpretation in the theory of contraction than in any theory depending on the action of sublunarian forces. For whenever an outer crust is forced to contract upon an enclosed nucleus, a tendency can be recognised to the formation of radially arranged corrugations. Nevertheless, it may be questioned whether—when this tendency is most clearly recognised—there is not always present some unyielding matter which forms a centre round which the radiations are formed; and it is somewhat difficult to see how or why such centres of resistance should exist in the case of the lunar crust. It is a little remarkable that here again we find ourselves led to entertain the notion that matter arriving from without has produced these sublunarian *knots*, if one may so speak, whose presence is not directly discernible, but is nevertheless strikingly indicated by these series of radiating streaks.

The circumstance already referred to, that these rays can only be well seen when the moon is full, has long and justly been regarded as among the most mysterious facts known respecting the moon. It is difficult to understand how the peculiarity is to be explained as due merely to a difference of surface-contour in the streaks; for it is as perplexing to understand how the neighbouring regions could darken from this cause just before full moon, and remain relatively dark during two or three days, as to explain the peculiarity by supposing that the rays themselves grow relatively bright. It is true that there are certain surfaces which appear less bright under a full than under an oblique illumination,—using the words "full" and "oblique" with reference to the general level of the surface. But the radiations occupy arcs of such enormous length upon the moon's surface, that the actual illumination of different parts of the radiations varies greatly, and of course there is a like variation in the illumination of different parts of the regions adjacent.

It is natural, under these circumstances, to inquire how far it is probable (1) that real processes of change take place month by month on the moon's surface, and (2) that it is to these processes that we owe the greater or lesser distinctness with which certain features present themselves.

It is known that Dr. De la Rue was led, by his photographic researches into the moon's condition (for we may fairly thus describe his experience in lunar photography), to the conclusion that processes resembling vegetation take place on the moon, the period during which the vegetation passes through its series of changes being a lunar month. He was particularly struck by the circumstance that portions of the moon which seem equally bright optically are by no means equally bright chemically. "Hence," he says, "the light and shade in the photograph do not correspond with the light and shade in the picture; and therefore the photograph frequently renders visible details which escape optically. Those portions of the moon near the dark limb are copied photographically with great difficulty, and it frequently requires an exposure five or six times as long to bring out those portions illumined by a very oblique ray, as others apparently not more bright when more favourably illuminated. The high ground in the neighbourhood of the southern portion of the moon is more easily copied than the low ground, usually called seas, and I have ventured to suggest that the moon may have an atmosphere of great density, but of small extent; and this idea has, I imagine, received some confirmation from a recent observation of Father Secchi's, of the lunar surface polarising light more in the great lowlands and in the bottoms of the craters, and not appreciably on the summits of the mountain-ridges."

It is extremely important to notice that photography shows the light near the terminator to be less bright than it appears to the eye. It may be, of course, that the distinction resides mainly or entirely between the photographic power and the luminosity of these portions; there may, for example, be an excess of yellow light and a deficiency of green, while the greater photographic power of the parts under full solar illumination may indicate an increase of green light due to some process of vegetation. It is, however, important to inquire whether the greater part of the difference may not be due to a physiological cause; whether, in fact, the neighbourhood of the dark portion of the disc may not cause the illuminated parts near the terminator to appear, through contrast, brighter than they really are.

On the answer which may be given to this question depends, in a great degree (as it seems to me), the opinion we are to form of those recent researches by Mr. Birt which have appeared to indicate that the floor of Plato grows darker as the sun rises higher above it. Taking these researches in their general aspect, it cannot but be recognised that it is a matter of the utmost importance to determine whether they indicate a real change or one which is only apparent. If it is really the case that Plato grows darker under a rising sun, we should have to infer that in the case of Plato certainly, and probably in the case of other regions similarly placed, processes of change take place in each lunation which correspond (fairly) with what might be expected if these regions became covered with some sort of vegetation as the lunar month (or, which is the same thing, the lunar day) proceeds. Other explanations—meteorological, chemical, or mechanical—might indeed be available, yet in any case conclusions of the utmost interest would present themselves for consideration.

It must be remembered, however, that thus far Mr. Birt's observations (as well those made by himself as those which he has collected together) are based on eye-estimations. Nothing has yet been done to apply any photometric test to the matter; nor has the floor of Plato been brought alone under observation, but other light, of varying degrees of intensity, has always been in the field of view. Plato is seen bright when near the "terminator," and growing gradually darker as the sun rises higher and higher above the level of the floor of the crater. The point to be decided is, how far the brightness of Plato near the terminator is an effect of contrast. De la Rue's photographic observations go far to prove (they at least strongly suggest) that contrast has much to do with the matter. He has shown that, photographically, the parts near the terminator are not so bright as they look. May it not be that they look brighter than they are in reality? We have only to suppose that De la Rue's photographic results represent pretty accurately the true relative luminosity of different parts of the moon to answer this question at once in the affirmative.

It seems to accord with this view, that the greater darkness of the floor of Plato agrees, according to Mr. Birt's light curves, with the time when the sun attains his greatest elevation above the level of the floor. For if the action of the sun were the cause of the darkening we should expect the greatest effect to appear some considerable time after the sun had culminated (as supposed to be seen from the floor of Plato). We know that on our own earth all diurnal solar effects, except those which may be described as optical, attain their maximum after the sun has reached his highest point on the heavens, while all annual solar effects attain their maximum after midsummer. If an observer on Venus could watch the forests of our north temperate zones as they became clothed with vegetation and were afterwards disrobed of their leafy garment during the progress of the year, it would not be on the 21st of June that he would recognise the most abundant signs of vegetation. In July and August vegetation most richly clothes the northern lands of our earth. It is then also that the heat is greatest; *that* is the time of true midsummer as distinguished from astronomical midsummer. And in like manner the true heat-noon is at about two o'clock in the afternoon, not at the epoch when the sun is highest, or at astronomical noon. The difference in either case amounts to about one-twelfth part of the complete period in question: in one case we find the maximum of heat a month or twelfth part of the year after the time of the sun's greatest northerly declination; in the other we find the time of greatest heat two hours or one-twelfth part of a day after the time of the sun's greatest elevation. If we take a corresponding portion of the lunar month, we find that the greatest effect of any solar action on the floor of Plato might be expected to take place about two-and-a-half days after the sun had attained his greatest elevation. This differs to a sufficient degree from Mr. Birt's estimate to justify the suspicion that either the effect is physiological, or that it is purely an optical peculiarity, that is, due to the manner in which the light falls on a surface of peculiar configuration.

It does not appear to me, I may remark further, that Mr. Birt has *demonstrated* the occurrence of real variations in the condition of the spots upon the floor of Plato. He has ascertained that some of these are at times relatively darker or brighter than at others, and that this is not a mere physiological effect is proved by the fact that the result has been obtained by comparing the spots *inter se*. Nevertheless it must not be forgotten how largely the presentation of the floor of Plato towards the terrestrial observer is affected by libration, now tilting the floor more fully towards the observer and presently tilting it away from him; at one time tilting the floor eastwards, at another westwards, and at intermediate periods giving every intermediate variety of tilt, —these changes, moreover, having their maximum in turn at all epochs of the lunation. Combining this consideration with the circumstance that very slight variations in the presentation of a flattish surface will cause certain portions to appear relatively dark or relatively light, it appears to me that a case has not yet been made out for those selenographical changes by which Mr. Birt has proposed to interpret these phenomena.

Nevertheless it cannot be insisted on too strongly that it is from the detailed examination of the moon's surface that we can now alone hope for exact information as to its present condition and past history. I would even urge, indeed, that the detailed examination at present being carried out is not sufficiently exact in method. I should be glad to hear of such processes of examination as were applied by Mr. Dawes to the solar spots. In particular it seems to me most important that the physiological effects which render ordinary telescopic observation and ordinary eye-estimates of size, brightness, and colour deceptive, should be as far as possible eliminated. This might be done by so arranging the observations that the conditions under which each part of the moon should be studied might be as far as possible equalised during the whole progress of the lunation. Thus, returning to the case of the floor of Plato: this region should not be examined when Plato is near the terminator as well as at the time of full moon, with the rest of the moon's disc or large portions thereof in the field of view; the eye of the observer should be protected from all light save that which comes from the floor itself; and, moreover, the artificial darkness produced for this purpose should be so obtained that the general light of the full moonlight should be excluded as well as the direct light from the disc. Then differences of tint should be carefully estimated either by means of graduated darkening-glasses, or by the introduction of artificially illuminated surfaces into the field of view for direct comparison with the lunar region whose brightness is to be determined.

When observations thus carefully conducted are made, and when the effects of libration as well as of the sun's altitude above the lunar regions studied are carefully taken into account, we should be better able than we are at present, as it appears to me, to determine whether the moon's surface is still undergoing changes of configuration. I cannot but think that such an inquiry would be made under more promising circumstances than those imagine who consider that the moon's surface has reached its ultimate condition, and that therefore the search for signs of change is a hopeless one. So far am I from considering it unlikely that the moon's surface is still undergoing change, that, on the contrary, it appears to me certain that the face of the moon must be undergoing changes of a somewhat remarkable nature, though not producing any results which are readily discerned by our imperfect telescopic means. It is not difficult to show reasons at least for believing that the face of the moon must be changing more rapidly than that of our earth. On the earth, indeed, we have active subterranean forces which may, perhaps, be wanting in the moon. On the earth, again, we have a sea acting constantly upon the shore—here removing great masses, there using the *débris* to beat down other parts of the coast, and by the mere effect of accumulated land-spoils acquiring power for fresh inroads. We have, moreover, wind and rain, river action and glacier action, and, lastly, the work of living creatures by land and by sea; while most of these causes of change may be regarded as probably, and some as certainly, wanting in the case of our satellite. Nevertheless there are processes at work out yonder which must be as active, one cannot but believe, as any of those which affect our earth. In each lunation the moon's surface undergoes changes of temperature which should suffice to disintegrate large portions of her surface, and with time to crumble her loftiest mountains into shapeless heaps. In the long lunar night of fourteen hours a cold far exceeding the intensest ever produced in terrestrial experiments must exist over the whole of the unilluminated hemisphere; and under the influence of this cold all the substances composing the moon's crust must shrink to their least dimensions,—not all equally (in this we find a circumstance increasing the energy of the disintegrating forces), but each according to the quality which our physicists denominate the coefficient of expansion. Then comes on the long lunar day, at first dissipating the intense cold, then gradually raising the substance of the lunar crust to a higher and higher degree of heat until (if the inferences of our most skilful physicists and the evidence obtained from our most powerful means of experiment can be trusted) the surface of the moon burns (one may almost say) with a heat of some 500° F. Under this tremendous heat all the substances which had shrunk to their least dimensions must expand according to their various degrees,—not greatly, indeed, so far as any small quantity of matter is affected, but to an important amount when large areas of the moon's surface are considered. Remembering the effects which take place on our earth, in the mere change from the frost of winter to the moderate warmth of early spring, it is difficult to conceive that such remarkable contraction and expansion can take place in a surface presumably less coherent than the relatively moist and plastic substances comprising the terrestrial crust, without gradually effecting the demolition of the steeper lunar elevations. When we consider, further, that these processes are repeated not year by year, but month by month, and that all the circumstances attending them are calculated to render them most effective because so slow, steadfast, and uniform in their progression, it certainly does not seem wonderful that our telescopists should from time to time recognise signs of change in the moon's face. So far from rejecting these as incredible, we should consider the wonder rather to be that they are not more commonly seen and more striking in their nature. Assuredly there is nothing which should lead our telescopists to turn from the study of the moon, as though it were hopeless to seek for signs of change on a surface so desolate. Rather they should increase the care with which they pursue their observations, holding confidently the assurance that there are signs of change to be detected, and that in all probability the recognition of such change may throw an instructive light on the moon's present condition, past history, and probable future.

IV. A SOLUTION OF THE SEWAGE PROBLEM.

THE treatment of sewage has long been an important question of Sanitary Reform. But the discussion involves a second question equally momentous,—that is, its utilisation, which is by no means implied in mere deodorisation or disinfection. A forcible illustration of this is found in Lord Palmerston's celebrated definition—"Dirt is matter in the wrong place." The offensive elements contained in sewage are in the wrong place when sent in to the river, but are in their right place when they are separated from it, and reserved, like the farmer's manure heap, for restoration to the land at the proper time. We invite epidemics if we permit the former; and we must cease to expect a fair supply of corn, wine, and oil, or the other bounties of Nature, if we neglect the latter, while we continue to draw from the land all its nutritive properties. The value of land is daily increasing, and therefore the highest possible cultivation becomes necessary. The only means of increasing its productive powers is by manuring, and for this purpose *all* matters possessing real fertilising value becomes a point of the first importance.

Many methods for dealing with the sewage of towns have been proposed. They may be classed under the four following schemes:—

1. Irrigation.
2. Filtration.
3. Destruction.
4. Precipitation.

These schemes may be considered individually or collectively in certain combinations.

Let us deal first with irrigation, and we may say at once that with us it has no favour, for it has been abundantly proved that at the best it is a disposal of sewage merely, and in no way its utilisation; for the excessively rank vegetation of a sewage farm forced to take more than is good is no more an evidence of high farming than was Wackford Squeers an evidence of the high feeding of the Yorkshire school. But even as a *disposal* of sewage it falls lamentably short of efficiency, as may be seen by any impartial inquirer. Under the most favourable circumstances this system is inadequate to deal with the entire sewage; for the quantity of land required annually to deodorise this (one acre for 100 people) is so large, in proportion to the land available for the purpose, that for financial, geological, and local reasons, the system could not succeed. There are other objections to irrigation with fluid sewage. Land for the purpose must be in propinquity to the town to which the system is applied, and this land may have to be bought in by the pressure of an Act of Parliament, at great expense, as it is generally opposed by wealthy landowners. Such opposition is to be expected; for the neighbourhood of a sewage farm would certainly not be selected by the rich as a site for their mansions; and the value of land is consequently deteriorated.

The charge of miasmatic emanations arising from a system of sewage irrigation has been abundantly proved by evidence given before the House of Commons by eminent medical and sanitary experts:—

Mr. Thomas Hawkesley, C.E., says (in reference to the Blackburn Corporation Improvement Bill, March 15th, 1870)—"Water irrigation carried on in warm weather is exceedingly unhealthy; in fact you make, so to speak, a kind of fen of the large area of land which you put the water over." . . . "Where the water is foul I can speak positively to it, from repeated observation in different places, that the odour, particularly at night, and particularly upon still damp evenings in autumn, is very sickly indeed, and that in all these cases a great deal of disease prevails; but I need not do more than upon that subject refer to the evidence taken by the General Board of Health itself." . . . "With regard to sewage irrigation this happens:—The sewage forms a deposit on the surface of the ground; that deposit forms a cake of organic matter; and that organic matter, when it is in a damp state, as it usually is, gives off in warm weather a most odious stench." Of the Barking farm Mr. Hawkesley says—"The stench was of a very fœtid character indeed, and of very considerable intensity." At Edinburgh, at Carlisle, and at Harrogate, the state of the atmosphere varies with the state of the weather. Of Edinburgh the witness says—"I cannot call it a mere odour in the ordinary sense. Everybody who walks down to Leith from Edinburgh, or to Portobello, in warm weather, cannot help being assaulted by it." At Carlisle, "they were utilising only about one-sixth of the sewage." At Croydon, where the soil is the most favourable that could be had, consisting of only a slight covering of alluvial matter upon chalk, gravel, and gravel-flints, "the people complain of this fœtid smell in summer, and particularly at night, and of a very low state of health in consequence;" and "the water does not run off clear," "nor nearly free from organic matter." At Birmingham, "It has a very prejudicial influence on the value of property." "Irrigation works with sewage water for the utilisation of sewage are most pernicious." Mr. W. E. Cressy, M.R.C.S., states, to the same Committee, that in the case of the sewage farm belonging to the Croydon Board of Works there has been, since 1867, typhoid fever in every cottage on the estate, which he refers to the existence of the farm. The water from the wells in the neighbourhood becomes putrid if allowed to stand for 24 hours. Cows feeding on the grass from this land yield milk which has been proved, by a series of experiments, to cause fever.

Dr. Henry Letheby, Medical Officer of Health to the City of London, gave evidence before the House of Commons in reference to both the Blackburn and Reading

Bills, on the 15th and 25th of March, 1870. He states that, taking the condition of the sewage put upon the land at Croydon, Norwood, Beddington, Rugby, Carlisle, and Worthing, the average proportions of matter in solution in the sewage, before it was put upon the land, was 32·77 grains. As it ran from the land it contained 34·3 grains, there being an increase in the solid matter after flowing through the land. The necessary conditions for irrigation, which he admits are not always present, are porous soil and good subsoil drainage. Frozen soil will not allow the sewage to sink, and a heavy rainfall will prevent it; and Dr. Letheby's experience has shown him that the land acts upon the sewage only at the time of active vegetation, "but that during the time of the dormant state of the vegetation the sewage runs off that land pretty nearly as it goes on it." He shows that, besides the acre of land for every 100 people, there must be another acre in reserve when that cannot be doing its work. The chief objections he considers to be, in the first place, the saturation of the soil with excrementitious matter, which is constantly giving off—sometimes to a great extent, at other times not so much—effluvia capable of producing disease. Secondly, "the subsoil water is always charged with decomposing matters, the residue of the sewage; and we know from the investigations recently of Dr. Pettinkoffer, who has examined into the question in England and Germany, and almost all over the world, that there is no more fruitful source of disease than a subsoil water charged with offensive matters, and altering in its level. The soil becomes filled with offensive gases, and he traces cholera and typhoid fever to these emanations, and he attributes epidemics to these emanations. Again, we have subsoil water which runs into the neighbouring wells, and whenever there is subsoil irrigation the neighbouring wells are offensive." . . . "There is another objection, which I look upon as the most serious of all: parasitic diseases in the human body are always derived from parasitic diseases in the flesh of the animals we eat. I hold in my hand a report from the most experienced man in this subject,—I may say in the world,—Dr. Cobbold. It treats of the more than probable, the certain, introduction of serious parasitic disease among the community, if sewage be put upon land as a means of utilising it."

These are the objections to the utility of the process of irrigation merely as a means of *disposal* of the sewage,—and they are very great,—whilst as we before observed, as to the equally important question of utilisation, its claims are very small indeed.

The abundance of crops produced on a given area has been quoted in favour of the system of irrigation. The finest manurial substances are possessed by the constituents of sewage; but the irrigationist is so wasteful in their application, that, in the majority of cases, there ensues not a healthy crop, but a mass of overgrown, rank grass material, of no more nutritive value than weeds; for be it distinctly remembered that this is not a question of manuring with sewage when necessary,—but the compulsory application of enormous quantities, in season and out of season, till the surfeited land is sick, and even then it has to take more still. If this waste were prevented, by the conversion of the sewage into a dry, portable, inoffensive manure, then this manure might be stored until it could be employed at the proper season without injurious effect; but to dose vegetation with equal quantities of manure, day by day throughout the year, is an absurdity which of itself is sufficient to condemn sewage irrigation.

The second process, that of filtration, appears to be involved in some obscurity,—that is to say, there are attached to the term several meanings, of greater or less comprehension. Not a little of the confusion appears due to the Rivers' Pollution Commission having discussed "intermittent downward filtration," without defining the term. We are told that irrigation owes no inconsiderable amount of its success to the contemporaneous effect of filtration of sewage through the soil, and, confusion worse confounded, we are instructed that "irrigation involves filtration." We, however, will take filtration to mean the passing of the sewage water through an artificially-constructed bed of sand, charcoal, &c. Filtration by itself is simply a method of disposing of sewage, not of utilising it, and therefore we hold it in no more favour than the other; for we maintain that unless the manurial elements are preserved *for* the land, as well as *from* the river, the problem is but half solved. Filtration processes do not profess to do the former, and as for the latter we do not find that they are very successful, so far as efficiency is combined with economy.

Let us revert, for an instant only, to the filtration—intermittent, or downward, or irrigation-filtration, or otherwise—of the Rivers' Commission. We will first describe the construction of such a filtering-bed, and will then take in consideration of the efficacy of this quasi-filtration the evidence of Dr. Frankland. The illustration is the construction of the Merthyr Tydvil beds, described by Mr. T. C. Scott, a strenuous advocate. "The filtering medium consists of 20 acres of land, drained 6 feet deep, and divided into four areas of 5 acres each. Each of these receives daily, for six hours out of the twenty-four, the sewage of 20,000 persons, represented by 900,000 gallons, or at the rate of 73,000 tons per acre per annum. To utilise advantageously, according to our present knowledge, the quantity of sewage thus dealt with, 200 acres of land are required, being at the rate of 1 acre for every 100 persons, or for 7,300 tons of this sewage." Now for the opinion of Dr. Frankland, who is an advocate of the irrigation system, and a Rivers' Pollution Commissioner. "I think it (this downward filtration) is an important part of our knowledge; but although I have had so much to do with it, I confess I am not very sanguine of its success as applied to large volumes of sewage, and for this reason: you collect upon the surface of your filters a large quantity of suspended matter from the sewage, which is fœcal matter in a state of decomposition, and we should be afraid that this matter so collected would be offensive to the neighbourhood. No plant can live upon the filter which is deluged in this way with sewage. This cannot be carried out along with plant growth, and consequently you have not the removal of those noxious constituents which accumulate on the surface by plant life, such as you have in irrigation." Thus, the filtration of unprepared sewage leaves us with far higher chances of miasma than do the evils of irrigation.

The process known as Weare's is a true filtration process, and is on a small scale said to be satisfactory. It has been employed in the workhouse at Stoke-upon-Trent; the filtration being effected through vegetable charcoal and fine ash, altogether a different method to the irrigation-filtration system. The filtering medium is placed in tanks through which the sewage percolates. The effluent water, however, still contains in solution a large proportion of putrescible organic matter, and is below the standard required by the Rivers' Pollution Commissioners, or by the Conservancy of the Thames.

But the evidence brought before the Parliamentary Committee on the Birmingham Sewerage Bill, in April and May last, has, we think, given the death-blow to sewage filtration. After fourteen days' hearing of the evidence of the leading authorities in Chemistry, Engineering, and Agriculture, the Select Committee attached to their approval of the Bill the condition that "*No sewage be put upon any land without having been previously defecated in tanks.*"

The third scheme of getting rid of sewage, viz., that of destruction, requires only a brief notice. By its very nature it forces us to condemn it: destruction has always been a favourite method of disposal of inconvenient elements, from time immemorial. Considerable difference of opinion exists as to what constitutes inconvenient elements,—but once admit the principle, and we find men using it to justify the murder of children by Lycurgus, and the monster fires of religious persecution. For our own part nothing will satisfy us but rational utilisation. Under the head of destructive processes we include the lime process of Tottenham and General Scott's cement process. By General Scott's process an effluent of a low standard of purity is obtained, whilst the result is in an agricultural point of view the most wasteful that could be devised. The sludge instead of being returned to the land is employed in the making of a kind of Portland cement. Human beings live directly and indirectly upon the produce of the land. Now scientific agriculture tells us that land unless properly manured becomes soon exhausted; and it is clear that the waste products of human beings, being most valuable in rendering the land fertile, should be returned to the land as manure, and not be destroyed. Few persons have any idea of the enormous waste which is committed in casting London sewage into the Thames. Mr. Mechi, a great authority on all agricultural subjects, tells us that the inhabitants of London consume *daily* the annual available produce of 20,000 acres, and a similar quantity is required weekly for London horses. The manurial wealth of this 20,000 acres of land is absolutely wasted, and the country thereby loses as much food as if three million quartern loaves were daily floating down the Thames towards the sea.

We come now to the consideration of the fourth scheme of defœcating sewage, by precipitating the solid constituents. The object of precipitation is to remove in a solid, dry, or semi-dry state the putrescible constituents of the sewage, and to render the filtrate or effluent water sufficiently pure to mingle with our streams, or be employed for purposes of irrigation. There are several processes which profess to remove more or less of the impurities from the water. Amongst these may be mentioned the Phosphate Sewage Company's process, the lime precipitation process, and the A B C process. In the Phosphate Sewage Company's process "the water is left still maintaining all its nitrogenous and valuable properties, *plus* any excess of phosphoric acid which has been added, and, therefore, highly useful for the irrigation of cereals and other crops." This process, which was invented by David Forbes, F.R.S., when compared with those with which we have been dealing, appears a very admirable one. It is, however, from a technological point of view, somewhat deficient in economy. For the ingredient added to the sewage is expensive, too expensive with regard to the return in practical good obtained from its use in agriculture; and unfortunately those plants which require most phosphoric acid bear irrigation least. In a theoretical point of view, if we overlook the infringement of the rule of economy, the process is attended with a high degree of consistency. By no means is the preceding statement true of all precipitation processes. The lime process, as an instance, produces a precipitate containing a large proportion of lime, possessing but feeble or no manurial power, and readily putrefying; while the effluent water, instead of being pure or even suited to the purposes of irrigation, contains introduced foreign matter inimical to the land and the life of plants.

The process of precipitating by sulphate of alumina the valuable constituents of sewage, and utilising at the same time the purifying power of charcoal and clay, is that to which we decidedly give the preference, as by this means the water is practically purified fit to be discharged into a running stream, and the deposit is retained in a form entirely inoffensive and capable of being turned into a dry and portable manure. This process has been before the world for some years as the A B C process, worked by a company called the Native Guano Company, and the claims it set up three years ago to have solved the great social problem we may now pronounce to be fully justified by facts; its principles were correct, the mechanical arrangements for conducting them being alone defective. The name of the process has been derived from the initial letters of the principal constituents of the precipitant: Alum, Blood, Clay, and Charcoal. We will consider the action of these substances upon sewage, taking them in order.

The alum was for a considerable time a source of expense, it being added to the sewage in the form of ammonia-alum. Ammonia-alum has the further disadvantage that the ammonia remains in the effluent water. A much more economical, and as effective a substitute has been found in a crude sulphate of alumina manufactured at one-fourth the cost.

The action of the sulphate of alumina may be briefly described.

In contact with sewage,—a slightly alkaline liquid charged with nitrogenous organic matter,—the alumina is separated in flocks, and, by virtue of its remarkable affinity for dissolved organic matter, each particle seizes hold of, and drags down with it, a corresponding particle of nitrogenous impurity. The blood here comes into play; this is essentially a liquid highly charged with albumen; albumen is instantly coagulated in the presence of alum; and in the same way as this ready coagulability of albumen is utilised in fining wine and coffee, so it is made use of in this process by joining with the alumina in its precipitation, uniting it in a net-work of fibres, and giving it, as it were, arms wherewith to seize upon and drag out of solution still more putrescible constituents.

But the precipitated hydrate of alumina is light in character, and although it would ultimately settle, leaving a clear liquid above it, the slightest agitation causes it to float up, and thus renders it difficult, on the large scale, to drain off the mud. At Paris sulphate of alumina has lately been employed for clarifying several hundred thousand gallons of sewage; and among the many defects of this process, that of imperfect settlement was by no means the least. Here the action of the clay is apparent. This substance has a curious physical property; when finely ground up with water it forms a creamy emulsion, which takes many days to settle; many rivers, in time of flood, owe their turbidity to this cause: the Seine at the present time is a striking example, its water being in colour, although not in actual impurity, as bad as the Thames below London Bridge. But when this creamy liquid meets with sulphate of alumina, the clay coagulates like albumen, and settles down in heavy granular flakes. Now in the A B C process these three precipitations—that of the alumina, that of the albumen, and that of the clay—take place simultaneously, and in each other's presence; they become closely locked together in a triple alliance; the heavy character of the clay particles gives density to the mass, and causes it to settle rapidly, and remain in a compact form at the bottom of the tank.

Were the object merely to produce an easily dried precipitate and a clear effluent, nothing more would be required; for not only has this precipitate carried down all the suspended matter, but much of the dissolved nitrogenous and albumenoid impurities have fixed themselves on to the alumina, whilst the clay has also performed its part in absorbing and carrying down a good proportion of the ammonia. But there still remains the probability, if not the certainty, of foul gases being present, whilst the water, though clear, may nevertheless be coloured. These residual impurities are attacked by the charcoal: the powerful affinity of animal charcoal for organic colouring matter corrects the one evil, whilst the well-known absorptive action exerted by vegetable charcoal on the gaseous products of putrefaction corrects the other. In the way of purification little more remains to be done.

These reactions, by a modification in the order in which the purifying ingredients are added, are effected at once, with a certainty of uniform results, and, by a simple mechanical arrangement, variation in the dose of each constituent required by a variation in the strength of the sewage can be readily controlled.

The method of applying the ingredients is extremely simple. The clay and charcoal are incorporated in a grinding mill with the aid of sufficient water to form a thin paste. This paste flows into a tank, and is constantly agitated until it is required to be mixed with the sewage. By the side of the mixing-room is a smaller room, through which passes a channel or trough. At one end of this channel there rushes in the London sewage, and with it an unmistakable odour. The B C mixture or thin water-paste of clay and charcoal is admitted to the trough by a pipe from the store-tank; the sewage in its passage past this pipe carries with it the mixture, and the two after well mixing proceed on their way past a second pipe connected with a tank containing a supply of sulphate of alumina dissolved in water. All that is now requisite is to allow the sewage, B C mixture, and alum to flow in inodorous company to the settling tanks. The channel leading to the tanks has its course interrupted by numerous ledges, which serve to cause the more perfect intermixture of the sewage and the disinfectants. The first tank in which the sludge is allowed to settle contains the principal portion of the precipitate. The clear water is allowed to flow off continuously from the first tank into a second tank; and the remainder of the mud is deposited in this and in the other tanks into which it flows. From the last tank the water is conducted to the river, appearing as a clear, inodorous, and tasteless effluent. When sufficient sludge has been collected in the first tank, the treated sewage is shut off from this, and permitted to flow into another tank, which then forms the first of the series. As much of the water as possible is then run off from the mud, and the latter is drawn into the acidifying tanks, where a small quantity of sulphuric acid is added to prevent the loss of any ammonia. From the acidifying tanks the semi-dry mud is pumped into the drying-presses, whence it issues in a cake. This semi-solid mud is then further dried by a most ingenious application of heat in revolving iron cylinders. The wet mud is passed in at one end, and dry manure, in the form of an inodorous and inoffensive powder, falls from the other end, at the rate of 5 tons in ten hours, at an expenditure of a few cwts. of coal.

If space enough be available the mud may be simply pumped from the bottom of the settling tanks into large open-air stanks, where it dries under the influence of the sun and air. Not the slightest offensive odour is apparent during any stage of this drying.

The dry mud in powder, and forming excellent manure, is removed from the sheds, and packed into bags for transport.

We have thus traced the process from the sewage to the manure and the effluent water. Before entering upon any statements with regard to the value of the results, we will more fully detail the process as it is followed at the experimental works at Crossness.

Crossness is situated on a projecting part of the southern shore of the Thames, between the Plumstead and Erith marshes, and is the southern outfall of the London drainage. The quantity of sewage now daily discharging at Crossness is 50,000,000 gallons. Large as this quantity may appear the enormous engines employed in pumping the sewage are fully equal to the task, for they are capable of lifting 280 tons in a minute, or nearly double the average flow. The transformation of such a mighty mass of filth into heaps of shining gold is a feat worthy of the days of the alchemist, or rather of the days of modern chemistry. Of this quantity of sewage the works of the Native Guano Company are capable of dealing in the twenty-four hours with 500,000 gallons, drawn from the cross-cut, or culvert through which the sewage runs into the principal reservoir. This quantity amounts to 1 per cent of the whole delivery. Thither the sewage flows into the sump of a pump worked by a 15 horse-power steam-engine, whence it flows into contact with the A B C constituents as we have described.

From the mixing trough the sewage, as described, flows to the settling tanks. These tanks are six in number, and are constructed of concrete, each being 50 feet long by 20 feet wide, and 8 feet in depth. When leaving the last settling tank the effluent water is caused to take a considerable fall, so as to afford room for the construction of a subway in such a manner as to place the sheet of water—as clear as plate-glass—between the visitor and the diffused light of the sky. In this position the transparency of the water is subjected to a most severe test, leaving no doubt as to the previous subsidence of all solid particles. The effluent water is run off to the Thames in a shallow brick-built conduit, about 4 feet wide by 270 feet in length, and arranged during its course to form several miniature cascades.

During an official trial, lately completed, extending over eighty days, there were used 80 tons of dry A B C materials, whilst the "native guano" obtained amounted, in the dry state, to 131 tons, showing an increase of more than 63 per cent. The amount of sewage treated during this time was 11,672,000 gallons. Therefore 1 ton of dry native guano was obtained from 89,100 gallons of the Crossness sewage.

The Crossness works are calculated to have cost the Company considerably more than it would be necessary to expend upon any works dealing with much larger quantities of sewage; but it is estimated that £5000 would amply remunerate the contractors for works which should deal with the sewage of 20,000 inhabitants, and that £1000 additional capital would provide for the working expenses. This, however, is not a matter with which we have to deal in detail.

The state of the effluent water may be viewed from two points—that of an analytical chemist, and that of a practical man of the world. The former can, without difficulty, make out a case which would lead persons ignorant of the weakness of purely chemical reasoning to condemn any water in the world; and a sensation is readily created by manipulating figures in such a way as to convert *grains* of the normal constituents of a good drinking water into *tons* of impurities, and by classifying perfectly innocuous substances under the fearful title of "previous sewage contamination."

Common sense leads one to judge of a water by other standards than those of theoretical chemistry. The effluent water from sewage purified by the A B C process, falling into the Thames at Crossness, into the Aire at Leeds, into the Croal at Bolton, and into the Seine at Paris, may not at all times come up to the fanciful requirements of a scientific chemist,—although the inhabitants of many towns and villages habitually use and thrive upon a worse water—but no intelligent man of the world will doubt its suitability for admixture with ordinary river water. It is perfectly limpid and colourless; it has no smell, and so little taste that were it not that the tasters know whence it comes they would not notice it. On standing, the water acquires no disagreeable odour; it forms no deposit, nor does it give rise to "sewage fungus" or other vegetable growth along the water-courses. Fish will live in it,—not only hardy varieties, but the more delicate kinds, such as gudgeon, to which a very slight taint of impurity is fatal. When the inquirer further finds that the effluent water is not too hard to interfere with its domestic use for washing or cooking purposes, he will endorse the opinion which the writer has deliberately formed, that there are not many English rivers on which large towns are situated which are as free from real impurity as the effluent water from sewage purified by this process.

Instead of fixing upon a fanciful standard of purity which could never be attained in practice, common sense decides that an effluent water from sewage is fit to be discharged into a running stream if it contain a less percentage of impurity than the water of that stream: the word "impurity" being not strained beyond its legitimate meaning, or made to include perfectly harmless constituents.

Let us now pass to the next point of inquiry—the manurial value of the "native guano," and the cost at which it is produced.

Of the value of a manure, chemistry can tell us little more than it can of the value of water. Just as mere chemical analysis would utterly condemn water containing Liebig's extract, infusion of tea, or a glass of bitter ale, as largely contaminated with nitrogenous organic matter or albumenoid ammonia; so chemistry, by taking a fictitious standard for manures, and judging only by the percentage of two of the many necessary constituents of the food of plants, gives an arbitrary money value to a manure, which is often exceeded by the price it fetches in the market. Agriculturists frequently pay more for nitrogenous and phosphatic manures than the price assigned to them by chemical analysis, and the sales of "native guano" form no exception to this rule.

In the autumn of last year the writer satisfied himself as to the alleged agricultural value of the manure, by personal enquiry amongst the farmers who had used it. With scarcely an exception, the farmers (of whom he saw twenty or thirty) were unanimous in their approval of "native guano:" many of them were shrewd, intelligent men, well acquainted with the various artificial manures in the market; they had tried "native guano" with intelligence on different fields against other manures, and were assured that—putting equal values per acre—it was superior to most manures in the market. Moreover, an examination of the books of the Company shows that the good opinion of agriculturists was genuine, inasmuch as a man who, the first year, would grudgingly take 1 ton as an experiment, the next year took 10 tons, and the third year would increase his order to 20, 50, and even 100 tons, grumbling that the limited supply prevented him having all he wanted.

But it must not be imagined that the results of the laboratory and of practice are altogether anomalous in the case of the native guano manure; there is simply a difference in degree, and this difference arises from the non-existence of a fixed chemical standard of manurial worth. Nor does chemical analysis always show a low money value for "native guano." Samples submitted, at the Paris works, to one of the first analytical chemists in France (M. Terreil, Aide-Naturaliste en Chef des Travaux Chimiques au Muséum d'Histoire Naturelle) are reported by him to be worth in their dry state 108·6 francs per ton, or, when reduced by the normal amount of moisture present in "native guano," and converted into English money, £3 12s. 5d. per ton, whilst the cost of production is far below that figure.

As more particular evidence of the manurial worth of the guano, we may refer to the results obtained on the experimental farm of 7 acres established in connection with the works at Crossness. The farm, as is indeed the entire system, has lately been under the supervision of the Metropolitan Board of Works. The following are the returns from 9 yards square:—

Golden Drop Wheat.

	lbs.	ozs.
Native Guano, 15 cwts. per acre . .	6	4½
Do. do. 10 do. do. . .	6	1½
No Guano	2	14

White Rough Chaff Wheat.

	lbs.	ozs.
Native Guano, 15 cwts. per acre . .	6	7
Do. do. 10 do. do. . .	5	7
No Guano	5	4

Revet Wheat.

	lbs.	ozs.
Native Guano, 15 cwts. per acre . .	11	0
Do. do. 10 do. do. .	10	14
No Guano.	6	8

Black Tartarian Oats.

	lbs.	ozs.
Native Guano, 15 cwts. per acre . .	6	8
No Guano	3	14

These results are worthy the attention of the farmer; but they are in no way surprising, for it is universally admitted that town sewage has manurial value; and as the ingredients of the A B C process which are added to the sewage have no *destructive* effect upon the constituents of the sewage, it would be a matter of much greater surprise if the "native guano" were found to be without manurial value. Further evidence in favour of the manure is, that there is a demand for it at the rate of £3 10s. the ton. That at Crossness the manure has cost more than this sum to produce is extremely probable, for the machinery, steam-engine, and tanks have been apparently arranged with the object of getting the minimum of work at the maximum of expense. Probably some of this is due to the necessity of erecting works before the most advantageous method of carrying on the process had been ascertained, whilst some of the apparent waste of money may be rendered necessary by the show character of the works, and the necessity of having everything aboveground to answer the accusation of improper dilution of the effluent. But when it is considered that fifteen times as much coal is being burnt there* as was sufficient for the same work at Paris; that the alum is costing three or four times as much as it need; that an experienced chemical superintendent is included among the staff; that the rest of the staff is about twice as numerous as need be; and last, though not least, that for the greater part of the three months' official trial, the sewage which has been treated has been excessively dilute, owing to heavy rains:—when all these extenuating circumstances are considered, the wonder is, not that the "native guano" produced at Crossness has exceeded £3 10s. per ton, but that the price has not risen to twice that figure. Let us turn to other works conducted on some approach to economical principles, and a very different result will be seen.

At Paris the expenses are higher than need be, owing to their being show works, and necessarily conducted with some disregard to economy. The works being simply for experimental illustration, were carried on intermittently, and were seldom in full operation, except when visitors were expected. The usual take of sewage was at the rate of 4800 gallons per hour, but on some occasions the working was pushed until the sewage was flowing at the rate of 10,000 gallons per hour. At this rate the precipitation and the settlement proceeded without difficulty, whilst the effluent continued to flow away without deterioration. Let us take the data of these works as the basis upon which to draw up a profit and loss account of a day's work.

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* This does not include coal used for artificially drying the "native guano" at Crossness.

Ten thousand gallons of sewage per hour amount to 100,000 gallons per day of 10 hours.

For this are required the following chemicals:—

	Kilos.		Kilos.		Frs.
Animal Charcoal . .	250	at 170 frs. per	1000	=	42·5
Vegetable Charcoal .	500	,, 50 ,,	,,	=	25·0
Clay	600	,, 6 ,,	,,	=	3·6
Lime and Blood . .	70	,, 12 ,,	,,	=	0·8
Sulphate of Alumina	162	,, 130 ,,	,,	=	21·0
Total used . .	1582	. . . . .	costing		92·9

The labour consisted of—

Four men, wages per day . . .	13·75 frs.
One superintendent	10·00 ,,
Add one extra man	4·00 ,,
	27·75 ,,

The steam-engine burnt less than half a ton of coals a week. This with a few sundries, such as oil, &c., amounted to about 36 frs. per week.

The mud was simply pumped from the bottom of the tank into an open-air stank, where it rapidly dried under the influence of the sun and wind, assisted by the porosity of the soil. The drying therefore cost nothing.

Owing to the excessive dilution of the Paris sewage from rainfall, from the copious street washings, and from the fact that most of the night-soil is carted away to La Villette, the yield of dry "native guano" was very poor, not more than 114 parts being obtained for every 100 parts of A B C materials added, as against 163 yielded under similar circumstances from London sewage. As 1582 kilos. of A B C materials were added, the "native guano" would be 1808 kilos.

The total expenses were—

Chemicals	92·90 frs.
Labour	27·75 ,,
Coal and Sundries	6·00 ,,
	126·65 ,,

As 1808 kilos. cost 126·65 frs., therefore 1000 kilos. would cost 70 frs., equal to £2 16s. per ton.

The value assigned to this manure was, as already stated, £3 12s. 5d. per ton.

Had the price been taken at which the clay alum can be made in England, viz., £2 per ton, instead of the French price, the expenses would have been still less per ton.

The writer has been allowed an opportunity of going through the accounts of the Hastings Works for the last six months. The cost of the "native guano" produced here averages £2 4s. 1d. per ton. The operations are not carried on as economically as they might be, and there are several serious items of current expense which would be avoided in subsequent works.

At Bolton, according to the certificate of the Mayor of the Corporation, who are themselves working the A B C process under a royalty, the manure is produced at a cost of £2 6s. per ton. The royalty derived by the Native Guano Company from the profits of the Corporation of Bolton amounts to 1 per cent of the entire capital invested by the Company; so that it requires but a few more applications to realise the permanent payment of a satisfactory dividend.

We are now in a position to make deductions from the evidence given before the House of Commons with regard to the value of the process.

Mr. Hawksley says:—"Now, the great virtue of this new method (A B C) is this, that while it is just as available as the old process of precipitation by lime, it produces a manure which can be sold to a profit, and the whole thing can be done in a moderate compass; and having been done in a moderate compass, of course it does not render it necessary to acquire a gentleman's estate by compulsion, or to produce these marshes which are injurious to the health of the neighbourhood. . . . The manure is now become of great value. . . . By this new process a valuable manure is produced, which sells at £3 10s. per ton, whereas the other manure (lime process) will only sell at from 1s. to 2s. 6d. per ton."

Dr. Henry Letheby says:—"The process is carried out at Leamington so satisfactorily that the effluent water is practically disinfected."

Dr. Frankland admits that he believes "the previous application of some chemical process, such as Sillar's (A B C) process, would entirely obviate that difficulty (the clogging of the filter) attending downward filtration."

There is one important property of the prepared "native guano" which we have still to notice. During the progress of the experiments at Leeds it was discovered that the "native guano," when made into a powder and mixed with night soil, absorbed all the moisture, thoroughly deodorised it, and rendered it a dry, inoffensive, and inodorous manure, capable of being easily transported without inconvenience. So valuable was this manure found to be that it was easily disposed of at £4 per ton, in quantities of 40 tons at a time.

From this discovery it followed that the A B C mixture should be employed to precipitate the colouring matters from refuse dye-waters of large dye-works. Some experiments were instituted in the laboratory, and the results were so satisfactory that the adoption of the process would fully answer the requirements of Mr. Stansfeld's bill for preventing the pollution of rivers.

The writer has thus endeavoured to give an outline of the A B C process of utilising sewage, to state, and to answer, objections to the process. The chief objections may be summarised as follows:—That the "native guano" is of no manurial value; this statement is untrue in fact. The writer has considered this objection very fully in a letter published some months since, a portion of which may be quoted here. "When manurial value is mentioned, a distinction must be made between the value assigned by chemical analysis and by actual experiment on a farm. The former method of valuation is most erroneous, as it only takes into account two constituents, and omits others of equal necessity to the plant life. Chemical analysis would assign scarcely any or no value to such substances as sulphate of lime, soot, the warp of the Humber, and the mud of the Nile; whilst, when a chemist does assign a value in money to a guano or a superphosphate, the price he fixes has little or no relation to the actual selling price. Farmers judge of its value by actual trial on their fields. It is in this way they fix the price it is worth their while to pay for the superphosphate, and in the same manner they judge of the value of 'native guano.' My observations at Leamington and the neighbourhood proved satisfactorily to my mind that the 'native guano' made there had a very high manurial value, and the farmers to whom I spoke about it had tested it in too many ways, and were too shrewd judges of such matters to be deceived in ascribing to native guano what was really due to previous manuring." The second objection is that the cost of the manure is more than £3 10s. per ton. In some experimental cases, perhaps, the cost has exceeded this amount per ton; but in cases where actual work has been commenced this amount has never been reached.

But let us for a moment suppose that no profit at all resulted from the sale of the manure; and that the sewage of London, we will say, had to be dealt with at the price of £2 per 100,000 gallons (and on the large scale it could certainly be treated at less than half this cost). We have then the sewage of London, amounting to 100,000,000 gallons per diem, treated (supposing the population to be 3,265,000) at 4s. per head per annum. The annual rateable property in the metropolis amounts, according to the Valuation Act of 1869, to £19,971,000. The cost of dealing with the whole of the London sewage could therefore be defrayed by a rate of 7-8ths of a penny in the pound. These facts are in themselves a sufficient recommendation of the process.

That the process should encounter opposition is not only possible but very probable. Its adoption will affect many vested interests, as well as the interests of rival schemes. But ratepayers, whether they be scientific men or not, would do well to investigate for themselves the claims of the A B C process. And not only the ratepayer, but every man who has a voice in the welfare of the nation and its production of food, or who desires that our towns should be healthy, should judge for himself of the value of the process. It may then be repeated that the claims of the A B C process to public confidence are threefold:—

I. It deodorises and disinfects sewage, and precipitates the suspended and much of the injurious dissolved matter without giving rise to any nuisance; it converts the deposit into a dry, portable, and inoffensive powder, possessing considerable manurial value.

II. It leaves the effluent water in a state of practical purity, fit to be discharged into any river.

III. It effects these important sanitary requirements at a cost, which not only relieves the ratepayers of expense, but even yields a profit, owing to the ready sale of the "native guano" at £3 10s. per ton, and its production at a cost of not more, and probably much less, than £2 a ton.

V. COLOURS AND THEIR RELATIONS.

By MUNGO PONTON, F.R.S.E.

PART I.

OF all the objects of perception presented to our sight in this beautiful world, none are more generally pleasing than colours. The brilliancy of some, the delicacy of others, their varieties of hue, of tint, and of shade, their melodies, so to speak, and their harmonies, all combine to render them sources of delight.

What would the landscape be without colour? Were it composed of only lights and shades, it would lose far more than half its beauty. It would be like a print compared with the glowing tints of a Claude or a Turner. What would be the plumage of the peacock without its gorgeous colours—its brilliant lustre, its playful hues? Let that ghost-like variety which is colourless say. And the most lovely of God's creatures—without colour, how would she appear? Where were the rosy cheeks, tokens of health—the coral lips—the many-hued iris, that index of the soul, with its deep yet lustrous browns, its ethereal blues, its tender hazels, its sagacious greys, with its margin of lucid white, the peculiar adornment of the human eye? And the wavy tresses too, with their tints in such strange sympathy with those of the iris—either in pleasing harmony or not less pleasing contrast. A woman of living alabaster, however elegantly formed, would hardly send a thrill of warmth through the frame of admiring man.

While colours thus afford pleasure to the eyes of the multitude, they awaken in the mind of the philosopher, who contemplates them with intelligent scrutiny, a still more exquisite delight. For he perceives in them evidences of most marvellous wisdom and skill, united to overflowing goodness and benign sympathy. When he considers the simplicity of the means, and the wondrous beauty and variety of the effects, he becomes lost in amazement. He feels himself, as it were, in the presence of a mind transcendently powerful, wise, and benevolent, so that his soul becomes filled with reverential, yet loving awe. For all these phenomena, which produce in him the varied and pleasurable perceptions of colour, are in themselves nothing more than variations in the rate of infinitesimally minute tremors, regulated by determinate mathematical laws.

The nature and minuteness of these vibrations, and some of their regulating laws, have been indicated in a previous essay, entitled "Molecules, Ultimates, Atoms, and Waves," which appeared in the "Quarterly Journal of Science," vol. i. N.S., p. 170, in April, 1871, and the two following numbers. In that essay reasons were adduced for concluding that the bright coloured lines observed in the spectra of glowing gases are due not to the vibrations of the ultimates of the gases themselves, but to those of more minute atoms constituting those ultimates. More especially in the case of hydrogen, it was shown to be probable that the ultimate of that gas consists of four species of extremely minute atoms, whose separate vibrations produce the four bright lines which characterise the spectrum of that gas when made to glow by passing through it an electrical discharge.

These views, respecting the constitution of hydrogen and the other chemical elements, have received a remarkable confirmation in certain phenomena observed by the spectroscope in the solar chromosphere. When viewed with that instrument, the chromosphere usually presents the four lines characteristic of hydrogen, and two other lines—one in the yellow, not coincident with the sodium lines, nor with any other produced by any known terrestrial substance, and denoted as D_3—the other in the red, a little less refrangible than C, and in like manner not referable to any known substance. Now, in two observations—one by Mr. Lockyer, the other by Professor Young (the latter made on 19th April, 1870), the line F, supposed to be due to hydrogen, was agitated in a remarkable manner, indicating that the substance in which this line has its origin was in a state of violent commotion; but on both occasions the red line C, also supposed to be due to hydrogen, remained totally unaffected.

Of this remarkable phenomenon the most simple explanation would be to suppose that, in the chromosphere, the four atoms constituting the ultimate of hydrogen exist disunited, forming four distinct gases more subtle than hydrogen; that of these gases, the one producing the line F was, during the observations in question, ascending in a gyratory column, while the one producing the line C was at rest.

A similar conclusion may be drawn from other spectroscopic observations of the solar limb, in which certain of the dark lines of the spectrum become converted into bright lines. It is remarkable that only a certain number of the lines due to particular metals have been thus affected—more especially three of the lines referred to magnesium, and only one or two of the numerous lines referred to iron. The lines thus altered were the three magnesium lines b_1, b_2, b_4, the line b_3 referred to iron and nickel, and the line 1474 of Kirchhoff's map also due to iron. But the strong magnesium line 5527 of Angström's scale and the numerous other iron lines underwent no similar change. It might be hence fairly inferred that, in the photosphere, the atoms constituting the ultimates of the various chemical elements, whose characteristic lines have been detected in the solar spectrum, all exist in a disunited state, forming an intimate mixture of highly attenuated gases; but that some of those gases occasionally pass the limits of the photosphere, and are projected a short way into the chromosphere, where they glow under the influence of electrical currents. For the bright lines, thus forming reversals of some of the dark lines of the photosphere, are always much shorter than the other bright lines of the chromosphere—a fact indicating that the substances which produce them ascend only a short way beyond the usual limits of the photosphere, in which these same lines are dark.

Subtle as, according to this view of their constitution, these gases must be, they must be excelled in their tenuity by others, which, extending beyond the chromosphere, form the corona seen in total eclipses of the sun. It is remarkable that the spectrum of the corona, as observed by Professor Young during the total eclipse of 1869, consists of three bright lines, so nearly coincident with those observed by Professor Winlock in the spectrum of the aurora borealis as to leave little doubt of their identity—thus indicating that the gases constituting the solar corona exist also in the region at the outskirts of the earth's atmosphere, where the auroral flashes play. These spectral lines of the solar corona and the aurora borealis have not yet been identified with any known spectra produced by artificial means. But the extreme lightness of the gases producing them renders it probable that, like the gases of the chromosphere, they consist of separate atoms not united into the ultimate of any chemical element. Should the individual lines be hereafter identified with any of those embraced in the spectrum of any known element or elements, this view of their constitution would be confirmed. (See Schellen's "Spectrum Analysis," pp. 361, 399, 404, 414).

The remarkable circumstance that, in the spectra of several of the nebulæ, there is seen only one of the bright lines of hydrogen—that, namely, corresponding to the line F—might in like manner be explained by supposing that, in these nebulæ, the atoms composing the ultimate of hydrogen are separate from each other, and that only those atoms yielding this particular bright line are in sufficient quantity to originate a light of such intensity as to penetrate through so great a distance as that at which these nebulæ are placed.

This particular phenomenon it has been sought to explain by the fact, observed by Messrs. Frankland and Lockyer, that when attenuated hydrogen is illuminated by the electrical discharge, and the spectrum produced is viewed at a considerable distance, all the bright lines disappear, save that corresponding to F. But this explanation is inconsistent with the fact that all the four hydrogen lines are distinguishable in the spectrum of the solar chromosphere. Moreover, the disappearance of all the lines but F from the spectrum of attenuated hydrogen, when viewed at a distance, is a fact which itself requires explanation; and the simplest is afforded by the supposition that, in each ultimate of hydrogen, the atoms which vibrate in unison with the line F considerably exceed in number those which give rise to the other three bright lines. It appears very unlikely that, were all the ultimates of hydrogen themselves individual atoms of the same bulk and weight, a greater number of them should elect to vibrate in unison with the F line than with the three other bright lines; while we should be left without any assignable reason why such atoms, if all exactly alike, should not, every one of them, vibrate in exactly the same time, and so give rise to only one bright line.

It has been recently pointed out by Mr. G. Johnston Stoney that the wave-lengths of the 1st, 2nd, and 4th hydrogen lines stand to each other approximately in the following relation—$20H_1 = 27H_2 = 32H_4$; whence he infers that these three may be harmonics derived from one and the same fundamental vibration (Phil. Mag., Aug., 1868). To this conclusion, however, is opposed the fact of the preponderance of H_2 or F over all the others; and still more the phenomenon already noted that, in two distinct observations on the solar prominences, the line H_2 or F was violently agitated, while H_1 or C remained unaffected. These two facts it appears impossible to explain, except on the supposition that C and F have their origin in two distinct sets of atoms, capable of existing either separately, constituting different gases, or united into one ultimate—that of hydrogen gas. In the sequel it will be shown on other grounds to be extremely improbable that these two have their wave-lengths in the exact ratio of $20C = 27F$. There remains, moreover, the fact that the vibrations corresponding to the line H_3 have no such numerical relations to the other three as these last have approximately among themselves, and exhibit no indication of their being derived from one and the same fundamental vibration with them. It appears, therefore, safest to conclude that the approximate numerical relation $20H_1=27H_2=32H_4$ is simply an indication that the inertiæ of these three sets of atoms stand to each other nearly in this relation.

It seemed advisable to make these explanations in reference to the former essay on "Molecules, Ultimates, Atoms, and Waves" with a view further to illustrate the subject of which it treats. It is now proposed to consider more at large the phenomena of colour simply as they present themselves to the eye, with their various relations.

Colours may be divided into two great classes—intrinsic and adventitious. Intrinsic colours depend on the arrangement of the molecules, ultimates, or atoms constituting the coloured substance; while adventitious colours depend on the disposition of aggregations of these into grains, fibres, layers, prisms; or they are due to the interference of wave with wave, the superposition of wave upon wave, the separation of wave from wave of the luminiferous ether; also in some cases to an alteration in the rate of vibration of the ethereal waves.

Intrinsic colours first demand attention, the phenomena which they present being comparatively few and simple. In the case of an elementary substance, which, while in the state of gas or vapour, exhibits colour, such as chlorine gas and the vapours of iodine and bromine, the colour most probably depends on the arrangement of the atoms constituting the ultimates of those elements. And this phenomenon furnishes a strong argument in favour of the view, that the chemical elements are really compounded of still more simple atoms. Did the colours of those elemental vapours depend on vibrations performed simply by their ultimates, seeing the vibrations would, in that case, be all of one rate, the tint produced would be one or other of the pure unmixed colours of the spectrum. But this they are not, consequently the vibrations causing them must be of various rates; nor does it appear possible to find any other cause of such a variation of rate than that of their being due to the compound nature of the ultimates—their consisting of atoms which, when set in motion by the ethereal waves, vibrate at different rates, producing a compound tint.

When the chemical elements are not in the gaseous or vaporous condition, their colour probably depends on the arrangement of the ultimates and their rates of vibration, rather than on those of the atoms constituting the ultimates. How much depends on the arrangement of the ultimates and their state of aggregation has been rendered evident by Faraday's experiments on gold leaf. This metal, when in very thin layers, is transparent, and the light passing through it is green; but by heating such films, and so altering the state of aggregation of the ultimates, the colour of the transmitted light becomes ruby-red. It can, however, be restored to green by simply compressing the layer. The light from the surface of the film in both cases retains its rich yellow hue and beautiful metallic lustre. In toning photographs with gold, however, the film, when extremely thin, is black, and not till the thickness of the deposit is augmented to an appreciable extent do the yellow tint and the metallic lustre return—the lustre preceding the tint in its reappearance, so that, at a certain stage, the surface presents a certain amount of metallic lustre while it is still black. Other metals besides gold exhibit variations of tint depending on the state of aggregation of their ultimates.

These phenomena bring us face to face with the question relative to the nature of intrinsic colours—the manner in which they are produced by the action of the ethereal vibrations. At one time it was generally supposed that the light falling on any coloured surface becomes separated into two portions, of which one is regularly reflected without change, the other scattered in all directions by the reflective action of the molecules or ultimates of the coloured surface, but deprived of some of its waves by absorption. Another opinion, however, has begun to prevail over this first notion. When it is remembered that what arrives at the coloured surface is simply motive energy, wafted onwards through the ether in waves of definite length, embracing vibrations of various rates, it will be perceived that if any of the motive energy of the ether disappear or become absorbed, it must be imparted to the molecules or ultimates of the surface on which the waves alight. These, again, cannot take up the energy without being themselves set a vibrating at the peculiar rates which they tend to assume. Moreover, the molecules or ultimates, on beginning thus to vibrate, must excite in the ether, in immediate association with them, fresh vibrations synchronous with those peculiar rates, and these will be propagated by undulations in all directions. It is this secondary set of ethereal vibrations which, according to the second view, produce in us those perceptions which we call the intrinsic colours of bodies. It is not a part of the incident light deprived of certain of its component waves, and scattered by reflection in all directions; but it is an entirely new set of waves owing their origin to the vibrations of the molecules or ultimates established by the motive energy of the incident light—these vibrations being of the same rate as those producing certain colours when they subsist in the ether.

Among other phenomena which favour this latter view is that presented by the scarlet geranium. It has long been observed that the colour of that flower continues to glow with apparently deeper intensity in the twilight. Now were the colour produced by the scattering in every direction of a portion of the incident light deprived of all its constituent waves, save those which combine to produce scarlet, the colour ought to become sensibly weaker as the incident light diminishes. But its appearing more intense, after the incident light has been greatly weakened, tends to prove that the scarlet colour is really produced by the vibrations of the colouring-matter of the petal—these vibrations subsisting for a considerable time after the stimulus of the incident light is lessened, and generating by their reaction vibrations in the ether synchronous with themselves. The apparent increase of intensity in the twilight is due to the circumstance that the scarlet colour is then less diluted with that portion of the incident light which is actually scattered in all directions from the surface of the petal during sunshine.

In the majority of cases the nature of the action is masked by the circumstance that the molecules or ultimates cease to vibrate almost immediately after the stimulus of the incident light ceases, though some wall-papers show their colours for a few seconds after the extinction of a candle, which has been placed near them. Nor is this owing to the mere persistence of the image on the retina; for it continues after the brighter image of the candle itself has disappeared. The same view is also strengthened by the phenomenon of lustre. For lustre is simply a portion of the incident light scattered from the coloured surface in every direction; but it is quite distinguishable from the coloured light of the surface itself, generated by the vibrations of the particles of which the coloured body is composed.

In all inorganic bodies the intrinsic colour is for the most part equably distributed over the surface or throughout the mass. In some chemical compounds the different ultimates constituting the compound molecule vibrate at different rates; but these become so blended as to produce compound tints; and they cannot be separated by submitting the substance to microscopical examination.

With organic bodies it is otherwise. Among these, the cellular structure more or less modified is so prevalent, that it is not surprising to find that their colouring-matter tends to accumulate in cells, which are easily distinguishable under the microscope. These are termed pigment cells. Even in cases where to the naked eye the tint appears uninterruptedly continuous and uniform, the microscope shows this apparent uniformity to be due almost entirely to the minuteness of the pigment cells and their close aggregation. Nothing can appear to the naked eye more uniform than the beautiful crimson tint of certain portions of the petal of the pelargonium, yet under the microscope the colour is seen to be accumulated in curiously-formed pigment cells. So, also, the skin of the negro, which to the naked eye appears of a uniform very dark brown, is seen when examined by the microscope to have its brown pigment accumulated in cells —some large and of a crescent shape, others much smaller and round. Another beautiful example is furnished by the minute sea-weed *Polysiphonia vestigiata*, which appears of a uniform red tint. Under the microscope the red pigment is seen to be accumulated in cells of an elongated form arranged in successive stages, a peculiarity from which the plant derives its name.

It has been mentioned that in the case of gold-leaf, the light transmitted through the film has a different colour from that which comes from its surface. This phenomenon, termed "dichroism," is exhibited by several other substances—silver-leaf, for instance, transmitting a blue light, while that proceeding from its surface is nearly white. The mineral termed dichroite or iolite, a prismatic quartz, is another example, its colour being deep blue when viewed in the direction of the axis of the crystal, and yellowish grey in the transverse direction. Crystals of augite, again, are blood-red in one direction and bright green in another. The alcoholic solution of chlorophyl, or leaf green, tinges the light passing through it of a deep red, while the superficial colour is green. In tincture of litmus the transmitted colour is also red, but the superficial is blue. The change from green to red in the instance of gold-leaf shows that, in some cases, these transmitted tints depend simply on the state of aggregation of the constituent ultimates. But in the tinctures of chlorophyl and litmus, the transmitted red is due to one of the constituents of those chemical compounds.

An interesting case is presented by the tincture of the bark of horse-chesnut, for it is one of transition. While dichroism may be regarded as intermediate between ordinary intrinsic colour and fluorescence, this tincture exhibits the transition between fluorescence and dichroism, another example of that tendency to gradation so conspicuous in many natural phenomena. In some varieties of fluor-spar there is dichroism combined with fluorescence, the green and blue fluor imparting to the light transmitted through it a green colour, while the superficial tint is deep blue. In the ate solution of the disulph~~ide~~ of quinine, again, there is fluorescence without dichroism, the transmitted light being colourless, and only the superficial light exhibiting the blue tint due to its fluorescent property. In uranium glass we have again dichroism combined with fluorescence, the transmitted light being yellow, the superficial fluorescent tint blue. But in the tincture of the bark of the horse-chesnut, when dropped in small quantity into water, there is a curious combination and succession of effects. The transmitted light is at first colourless, as in the case of the quinine solution, while the superficial tint is blue, but deeper than that proceeding from quinine. In a short time, however, the transmitted light in the case of the horse-chesnut bark acquires a straw-colour, which gradually deepens, the blue fluorescence still continuing without much diminution, so that we have again dichroism and fluorescence combined. Ultimately, however, the solution, though exceedingly weak, acquires the tint of brown sherry as respects both the transmitted and the superficial light, the fluorescent blue having gradually died away.

Fluorescence itself forms the transition between intrinsic and adventitious colour. There can be no doubt that this phenomenon is caused by the vibration of the molecules of the fluorescent body. The peculiarity is that this motion may be established by ethereal waves lying beyond the limits of the visible spectrum. The most remarkable case is that presented by the extremely minute ethereal waves proceeding from aluminium electrodes, which, as has been shown in a previous essay already referred to, are very far removed from the visible spectrum beyond its violet extremity—beyond even the limits of actinic action. Yet these minute waves can excite in the phosphate of uranium vibrations which, in their turn, originate fresh ethereal vibrations lying within the limits of visibility. It is thus rendered evident that the vibrations excited in the uranium salt are very much slower in their rate than are the ethereal vibrations by which they are established, and that these uranium vibrations in their turn give rise to fresh ethereal vibrations synchronous with their own slower rate, and capable of exciting the optic nerve. The case resembles that of a bass string set a vibrating by the vibrations of a treble string several octaves higher in the scale.

This phenomenon affords evidence that the molecules of bodies are actually made to vibrate by the ethereal waves, and do in their turn propagate a secondary set of ethereal vibrations—so far favouring the second view of the nature of intrinsic colours. Indeed, according to this view, the only difference between fluorescent colours and ordinary intrinsic colours consists in this circumstance, that, whereas the latter are due to vibrations established in the molecules by ethereal waves lying within the limits of the visible spectrum, the vibrations causing fluorescence are established by ethereal vibrations more rapid—sometimes greatly more rapid than themselves. If the incident light be winnowed from all waves of shorter period than the green, there is no fluorescence; and in this sense the fluorescent tint may be regarded as adventitious; because it depends for its exhibition on the character of the incident light. But it is in another aspect intrinsic; because it depends on the molecular vibrations of the fluorescent body.

The flame of a spirit-lamp, though deficient in light capable of stimulating bodies to exhibit their intrinsic colours, abounds in that sort of light which stimulates fluorescent bodies. If in a dark room a spirit-lamp be lighted and placed behind the observer, and if he put on a smooth black surface a drop of water, and alongside of it a drop of a weak solution of the disulphide of quinine, or of the bark of the horse-chesnut, and examine these by placing them near the level of the eye, while the drop of water will be hardly visible, that of the fluorescent liquid will appear quite solid and of the colour of a turquois.

Another phenomenon, illustrating the great influence of the molecular condition of bodies upon the light falling on them, is that of temporary colours. The most familiar example of these is furnished by the sympathetic inks formed by the chlorides of cobalt and nickel. Very dilute solutions of those salts are so nearly colourless that when laid on paper they are invisible. But when subjected to heat the former becomes blue, the latter yellow, while by combining the two a green is obtained. These colours gradually disappear when the paper stained with them is exposed to the air; but they may be restored again and again by mere warmth. The explanation is that the heat drives off all moisture from the salts, and their molecules when dry tend to vibrate—the one in unison with the blue, the other with the yellow ray, when exposed to light. On the withdrawal of the heat the salts again imbibe moisture from the air, and their molecular vibrations, under the stimulus of the incident light, have no longer these definite rates.

The temporary effects of heat on nitrous acid gas may be classed under this same head. This gas, even at ordinary temperatures, exerts on the incident light a strong absorptive action, in virtue of which numerous dark lines are developed in the spectrum; but raising the temperature of the gas so increases this absorptive power as ultimately to convert the whole of the incident light into dark radiant heat—the gas becoming quite opaque. A fall of temperature allows it to resume its transparency. In this case, the heat tends to cause the molecules of the gas to take up the vibratory energy of the incident light, and in virtue of this energy, united to that of the applied heat, to perform vibrations of so great an amplitude and so slow a rate that they do not in their turn communicate to the ether back-waves of a rapidity sufficient to affect the optic nerve. These back-waves accordingly assume the form of dark radiant heat. When the temperature is lowered again, the molecules perform vibrations of smaller amplitude and greater rapidity, which in their turn propagate through the ether back-waves of such rates as to develop colours belonging to the red end of the spectrum.

PART II.

Intrinsic colours having been considered in the previous part, the present shall be devoted to those called adventitious. Of such, the most simple sort are those produced by dispersion, or the separation of wave from wave of the incident light. In this case, the medium by which the separation is effected may itself be destitute of colour. All that is requisite is that it should be shaped into the form of a wedge or prism, so that the incident light shall pass through varying thicknesses. The diverse waves, of which the incident light consists, are thus subjected to the retarding action of the medium for different periods of time; and they are accordingly turned aside out of their direct course, or refracted in unequal degrees. Those most easily retarded become thus separated from those least easily retarded, and the waves of different lengths reach the eye in this separate condition, producing each its distinct impression of colour. All refracted spectra are of this character. The colours do not belong intrinsically to any substance or object. The eye which is usually impressed simultaneously by luminous waves of every degree of length, causing the perception of mere brightness, is in this case separately impressed by waves of different definite lengths, the vibrations of which are so adjusted to those of which the optic nerve is capable, as to excite in us the perception of definite colour.

The laws which regulate the dispersion of light in passing through diverse media are exceedingly curious; and some of the more important of them have been noticed in a previous essay on the spectroscope in the "Quarterly Journal of Science" for January, 1872.

A familiar example of the production of adventitious colours by the separation of wave from wave of the ether, where the object which affects the separation is itself colourless, is exhibited in the rainbow. This phenomenon is produced by the action of falling rain-drops, or of the spray from waterfalls on the sunbeams. The ethereal waves, on entering a rain-drop, become separated one from another, owing to their unequal refrangibility, and their passing through different thicknesses of the watery medium. Being reflected from the posterior surface of the drop in this separate condition, they undergo further separation in passing a second time through the water; so that, on emergence, the differently coloured waves reach the eye separately. The mode of formation of the primary and secondary bows will be found explained on mathematical principles in the "Edinburgh Encyclopædia," vol. xv., p. 616.

Haloes round the sun and moon are also examples of the same sort of adventitious colours, and their explanation depends on similar principles; only the objects by which the separation of the ethereal waves is effected are thought to be not rain drops, but minute frozen particles of water. It is supposed that a stream of air, charged with moisture at a low temperature, comes into contact with a denser, drier, and colder stratum, by which the particles of moisture become suddenly frozen into very minute crystals, which are sustained floating in the atmosphere at a considerable height, in a thin semi-transparent layer, forming a sort of veil between the observer and the sun or moon. The explanation of these phenomena on mathematical principles will be found in the "Edinburgh Encyclopædia," vol. x., pp. 616, 617.

To similar causes are to be attributed the rarer and more striking phenomena of the parhelion or mock-sun, and the paraselene or mock-moon. The author, many years ago, once enjoyed an opportunity of seeing a parhelion of great beauty in the north of Scotland, and he still retains a lively recollection of its aspect. The phenomenon varies considerably, but in general it may be said to consist in the formation of several large luminous rings or arches, sometimes coloured, sometimes only bright, at some distance from the sun or moon, and intersecting each other at two or more points—the points of intersection being usually occupied by the mock-sun or mock-moon. Sometimes there are only two of these spectral images of the luminary—one on either side of the true disc, and at a considerable distance from it. In other instances there are three or four—more rarely six such spectral images. To this latter category belonged the remarkable parhelion seen by Scheiner in 1630, of which a particular description was handed down by Gassendi, the astronomer. See the "Edinburgh Encyclopædia," vol. x., p. 613, where several other forms of the phenomenon are described. In that of 1630 there was one complete luminous ring around the sun, another much larger passing through the disc of the luminary, a third of nearly the same size surrounding the sun, but of which the lower third was invisible; while there was a portion of a fourth touching the upper limit of the third, and stretching thence upwards a short way towards the zenith. Of the spectral images of the sun, four were situated in the large ring passing through his true disc. They were formed at the points where this ring was intersected by the other two, which had the true sun for their centre. The fifth image was situated right over the true sun, on the margin of the innermost of those two surrounding rings; while the sixth was situated also right above the true sun at double the distance from his disc, on the margin of the second surrounding ring, at the point where it was cut by the fragmentary ring at its summit. The spectral images seen by Scheiner continued visible for upwards of four hours.

The second case of adventitious colour is that due to the interference of one luminous wave with another—the two proceeding from very closely approximated surfaces. The system of rings, named after their discoverer, Sir Isaac Newton, presents this phenomenon in its simplest form. To obtain these in perfection, it is necessary to place a long focused convex lens against a little longer focused concave lens, and to exhaust the air from between them, so as to press them very closely and equably together by atmospheric pressure. The colours of the reflected and transmitted light are in every case complementary to each other, being such as would, by their union, produce white light. When light of one pure colour is thrown on the lenses, the rings are all of that one colour, and merely light and dark—the waves alternately doubling and extinguishing the effects of each other. Their breadth is greatest with red and least with violet light; while it is by the overlapping of these rings and the consequent intermingling of their tints that the succession of colours is produced when white light is employed.

Another method of exhibiting these beautiful rings is by blowing soap bubbles of a large size. This may be done by using a mixture of soap and glycerine, and the bubbles thus obtained may be preserved for several hours intact under a bell-glass. The colours are here produced by the interference of the light coming from the inner surface of the film with that coming from its outer surface. The two surfaces are most nearly approximated at the summit of the bubble, and they gradually separate thence downwards, so that the same conditions are present as in the case of the two lenses. Another simple way of producing this system of rings is by spreading a thin film of soap over a glass plate, and breathing on it through a finely pointed metal tube. In this case the effect is due to the condensation of the breath into minute hollow vesicles, which increase in size from the centre outwards. They are, in fact, diminutive soap bubbles.

This class of colours goes under the general denomination of the colours of thin plates, and the colours of many natural objects fall under this category. Among the most beautiful of these, and the most nearly allied to Newton's rings, are the colours exhibited by the discoid frustules of certain of the Diatomaceæ. These consist of very thin superimposed plates of pure silica, ornamented with various patterns, produced by extremely minute papillary projections.

To this same class belong the colours seen in the scum floating on the surface of some liquids, especially of solutions containing salts of iron; also the colours of fibres and of feathers very generally. The colour of some feathers, however, are intrinsic, consisting of colouring-matter lodged in pigment cells, whence it can be removed and separately examined. The most interesting case of the kind is that of the red feathers in the wings of the plaintain-eater (*Musophaga violacea*) and the turacu (*Turacus albocristatus*), which owe their red colour to a pigment that has been named turacine. This pigment possesses dichroism, being of a deep violet purple by reflected light and crimson by transmitted light. It presents the great peculiarity of containing nearly 6 per cent of metallic copper, which must have entered with the food or drink of the bird, have passed through its circulation, and found its ultimate lodgment in those wing-feathers. It is most abundant at the pairing season (see an interesting paper on the subject in "The Student," vol. i., p. 161, where will be found a chromolithograph of the two birds above named).

All iridescent colours in fibres or spines are adventitious, and belong to the class of colours of thin plates; as, for example, the beautiful iridescent spines of the sea-mouse (*Aphrodite aculeata*), and the iridescent branchiæ of the *Eolis*, which serve the double purpose of a breathing apparatus and a bank of oars. The colours of the wings of insects and the elytra of beetles, &c., all fall under the same extensive category.

The iridescence of mother-of-pearl and the fire of the opal, again, though also phenomena of interference, may perhaps be regarded as rather transitional in their character, approaching towards the colours developed by systems of fine lines, producing the phenomena of diffraction. The simplest case of diffraction is that of the external and internal fringes, developed when a single thin obstacle, such as a fine wire or a very thin opaque plate placed edgewise, is set in the path of a divergent beam of sunlight. In this instance the internal fringes are produced by the overlapping of the waves bent inwards from the opposite sides of the obstacle; while the outer fringes are due to secondary waves propagated from the outer edges of the obstacle, which interfere with the direct waves coming from the luminous source. It is by a system of extremely fine and very closely approximated equidistant lines that the diffracted spectrum—the purest of all spectra—is produced.

By far the most beautiful exhibition of adventitious colours is that to be obtained by means of polarised light, or light consisting of waves, the vibrations of which are all performed in one plane. To produce the phenomena of colour in this manner, it is needful to have the means of polarising the light in two opposite planes—the plane in which the vibrations are performed in the one set of waves being perpendicular to that in which they are performed in the other set. The light may be thus polarised either by reflection from a smooth surface at a certain angle, or by means of crystals of Iceland spar, cut so as to form what are called, from their inventor, "Nicol's prisms," or else by means of a plate of tourmaline or of iodide of quinine. Of these appliances one is used for polarising the light, the other as an eye-piece for analysing it, that is to say, for showing that it is polarised, and for indicating the character of its polarisation. The colours are developed when certain crystals and also certain organic substances are interposed between the polariser and the analyser. In passing through these interposed media, the light is more or less depolarised, while the depolarising energy acts unequally on the different waves, and is manifested unequally in different parts and directions when the interposed medium is a crystal. The result is the greater or less separation of the differently coloured waves one from another, and that in such a manner as, in many instances, to display the intimate internal structure of the crystal, or other depolarising substance.

The combined action of different colours when they fall simultaneously on the retina is curious. An interesting series of experiments, with a view to illustrate this action, has been made by Prof. J. Clerk Maxwell, who has communicated the results to the Royal Society in a paper published in the "Philosophical Transactions" for 1860. By an ingenious apparatus he contrived to bring three diverse pure colours of the spectrum to bear on one point of the retina. He ascertained that there is in the spectrum a central point, which he describes as being about a fourth from E towards F. This would make its wave-length on Angström's scale 5156·72. As Prof. Maxwell determined this point by means of two flint glass prisms, allowance must be made for their irrationality; so that in all probability the exact position of the central point is, in the normal spectrum, the mean green ray, of which, as will be afterwards shown, the wave-length is 5124·086 (reciprocal 1951·568). Prof. Maxwell has detected a curious peculiarity of the rays at and near this point, namely, that at the *punctum cœcum*, or yellow spot in the retina, there is a greater insensibility to these rays than to any others of the spectrum.

By causing the rays from this central green point to fall on the retina in conjunction with the rays from some point in the red, Prof. Maxwell found that colours undistinguishable from the intermediate pure orange and yellow of the spectrum could be produced, the only difference being that these compound tints are resolvable by the prism into their constituent elements, while the pure tints of the spectrum are not. In like manner it is always possible to select two colours from somewhat distant points of the spectrum, which will, when combined in certain proportions, produce intermediate tints undistinguishable from one or other of the remaining pure tints of the spectrum.

The most remarkable effects, however, are those produced by causing the rays from three points in the spectrum to fall on one point of the retina,—the result being the impression of pure white, undistinguishable from the white resulting from the combination of all the spectral colours. The proportions required to constitute this white vary with the points on either side of the central green from which the rays are taken. It is impossible by mixing pigments to produce a similar result. If carmine, chrome-yellow, and indigo be mixed in certain proportions, the resulting impression on the eye is that of blackness, not of whiteness. It is possible, indeed, by whirling a disc, painted with different proportions of red, green, and blue, to produce a greyish-white, but not that pure white which may be obtained by combining the rays of the spectrum.

Prof. Maxwell extended his observations to the case of colour-blind persons. The eyes of those whom he examined were dichromic,—that is, sensible of only two impressions of colour. The central green of the spectrum appeared to them white, as did a considerable extent on either side of it. Beyond that, on the less refrangible side, all appeared of one colour, which they termed yellow of different degrees of intensity, shading off into darkness towards the red extremity; while on the more refrangible side all appeared likewise of one colour, which they called blue of different degrees of intensity, shading off into darkness at the violet end. The space from the fixed line A to E appears yellow, reaching its maximum between D and E, while the blue reaches its maximum at about two-thirds from F towards G. The mean green ray produces a fainter impression on the *punctum cœcum* in such eyes than in those of more perfect visual power. In the dichromic eye, rays taken from different points of the regions on opposite sides of the central green, when combined in certain proportions, produce the impression of whiteness without the aid of a third ray. But no admixture of blue and yellow will to such eyes appear green. Any combination of these two will appear white,—either a yellowish-white if the yellow be in excess, or a bluish-white if the blue predominate.

These experiments throw great light on the nature of complementary colours. They show that to perfect eyes, when two colours are complementary, one or both of them must be compound colours, and that only in dichromic eyes can two pure colours be regarded as complementary. To such eyes, yellow and blue being the only colours distinguishable, are always complementary to each other. When a perfect eye, however, after dwelling for a long time on a pure red, is turned on a pure white, the complementary green which it sees is not pure, but is that mixture of blue and green which is needful to complement the pure red, in order to constitute a perfect white. So, when the eye first dwells on a pure green, the red which it subsequently sees is not pure, but that mixture of red with blue which, in order to constitute a perfect white, is needful to complement the pure green. From these observations it follows, that in Newton's rings, the reflected and refracted tints, being complementary to each other, cannot be pure colours, such as are those of the diffracted spectrum; but there must be at least three pure colours in every opposing pair of the Newtonian rings.

From the foregoing sketch it will be perceived what an additional charm has been thrown around the subject of colour by the discoveries of Natural Philosophy. By the appliances of which that science avails itself we are, as it were, furnished with additional organs of vision, and enabled to contemplate natural beauties, of which the human mind had, before those discoveries, hardly formed a conception. And then there returns upon us the startling fact, that all these wonderful and beautiful phenomena are nothing more than mere variations in the rates of certain minute vibrations,—just as are the notes of various musical instruments in the case of sound, whose melodies and harmonies have thus, to a certain extent, their analogies in those of colour. The nature and scope of these analogies will be considered in the remaining part of this paper.

PART III.

The analogy between colours and musical tones has presented itself to many minds, and there has been among scientific men much discussion as to its nature and extent. The grounds on which those who have contended for a perfect correspondence between the colours of the spectrum and the notes of the musical scale have based their argument, were at one time supposed to be stronger than they actually are.

The case is greatly complicated by the uncertainty which prevails in regard to what really constitutes the true musical scale. The mathematical idea of a perfectly musical scale is one that should divide the octave into twelve equivalent semitones, forming a regular geometrical progression. For the purpose of comparison with the actual musical scales, this ideal scale is here given, with the relative number of vibrations referred to those of *do* as unity, and with the logarithms of these numbers, the common ratio of the progression being the twelfth root of 2.

Ideal Scale.

Names of Notes.	Ratios of Vibrations.	Logarithms.	Com. Dif.
Do	1	0·0000000	0·0250858
Do♯ re♭	1·059463	0·0250858	
Re	1·122462	0·0501716	
Re♯ mi♭	1·189207	0·0752574	
Mi	1·259921	0·1003432	
Fa	1·334839	0·1254290	
Fa♯ sol♭	1·414213	0·1505148	
Sol	1·498306	0·1756006	
Sol♯ la♭	1·587400	0·2006864	
La	1·681792	0·2257722	
La♯ si♭	1·781796	0·2508580	
Si	1·887747	0·2759438	
Do_2	2	0·3010300	

Thus constituting a regular geometrical progression.

Had the earliest musicians been also mathematicians it is not improbable that this is the scale they would have adopted; while so great are the powers of habit and inheritance on man's mind and organisation that it would, in the course of time, have come to be regarded as the true scale, the succession of its notes as perfect melody, their combinations as perfect harmony. The state of the fact, however, is quite otherwise. Melody and harmony have become, to a certain extent, dissociated, and the scale which is regarded as yielding the most perfect melody differs from that which is regarded as yielding the most perfect harmony,—neither of them, however, forming regular geometrical progressions, consequently both differing considerably from the ideal scale.

In the Pythagorean scale, which yields the most perfect melody, the *sol* is regarded as occupying the exact middle point between *do* and its octave do_2; consequently the ratio of its vibrations referred to *do* as unity is 1·5. From these three, *do, sol,* do_2, all the other members of the scale are derived by multiplication or division. The principal notes are found thus :—$Sol^2 \div do_2 = re$, $re^2 = mi$, $sol \div re = fa$, $mi \times fa = la$, $mi \times sol = si$. The sharps thus :—$Fa \div mi = do\sharp$, $sol \div mi = re\sharp$, $re\sharp^2 = fa\sharp$, $re\sharp \times fa = sol\sharp$, $fa^2 = la\sharp$. The flats thus :—*Mi* ÷ *re*♯ = *re*♭, *la* ÷ *fa*♯ = *mi*♭, *re*^{3} = *sol*♭, *re*^{4} = *la*♭, *re*^{5} = *si*♭.

This scale, with its logarithms and their differences, stands as follows :—

Pythagorean Scale.

Names of Notes.	Ratios of Vibrations.	Logarithms.	Differences.	Differences.
Do . .	1	0	0·0226335	
Do♯ . .	1·0535	0·0226335	. .	0·0058856
Re♭ . .	1·067872	0·0285191	0·0226335	
Re . .	1·125	0·0511526	0·0226335	
Re♯ . .	1·185185	0·0737861		0·0058856
Mi♭ . .	1·201356	0·0796717	0·0226335	
Mi . .	1·265635	0·1023052	0·0226335	
Fa . .	1·33′	0·1249387	0·0226335	
Fa♯ . .	1·404663	0·1475722		0·0058856
Sol♭ . .	1·423829	0·1534578	0·0226335	
Sol . .	1·5	0·1760913	0·0226335	
Sol♯ . .	1·580207	0·1987248		0·0058856
La♭ . .	1·601808	0·2046104	0·0226335	
La . .	1·6875	0·2272439	0·0226335	
La♯ . .	1·77′	0·2498774		0·0058856
Si♭ . .	1·802034	0·2557630	0·0226335	
Si . .	1·898438	0·2783965	0·0226335	
Do$_2$ . .	2	0·3010300		

This is the scale according to which the violin is tuned and played, except when it accompanies a keyed instrument. It will be observed that the sharps and flats are here separated, and this distinction is recognised by all good violinists. The geometrical progression in this scale is far from perfect, but the irregularities are recurrent and nearly symmetrical. It has been shown, moreover, by MM. Cornu and Mercadier, that this scale agrees very closely with observation, when a violin is made to register automatically the vibrations of its strings. (See "Nature," vol. iii., 75).

It is found however, in practice, that the *mi* of this scale, when struck along with the *do*, does not produce perfect harmony, and that, to obtain a harmony free from beats, the *mi* must be lowered by a small interval called a comma, its value being 81÷80. This alteration in the value of *mi* involves an alteration in several other notes of the scale, in order to obtain good harmony; while it is also found most convenient to throw the adjacent sharps and flats together into one note, as they exist in the ideal scale, for better adaptation to keyed instruments.

In the construction of this harmonic scale the same three notes, *do*, *sol*, do_2, are, as in the former case, assumed as a basis, *re* and *fa* being derived from them in the same manner as before. But the other notes are deduced from these on a different principle from that which is followed in the preceding case. There are formed three arithmetical progressions—*do mi sol*, *do fa la*, *re sol si*—the first having a common difference of 1-4th, the second of 1-3rd, and the third of 3-8ths, while from these *mi*, *la*, and *si* are respectively derived thus:—

$$Mi = do + \frac{sol - do}{2},\ la = do + \frac{fa}{2},\ si = 2\,sol - re.$$

The chromatic members of the scale are found thus:—*Fa* ÷ *mi* = *do*♯ or *re*♭, *sol* ÷ *mi* = *re*♯ or *mi*♭, *re* × *mi* = *fa*♯ or *sol*♭, *fa* × *mi*♭ = *sol*♯ or *la*♭, fa^2 = *la*♯ or *si*♭. The following is the scale thus constructed, with its logarithms and their differences:—

Harmonic Scale.

Names of Notes.	Ratios of Vibrations.	Logarithms.	Differences.	Differences.	Differences.
Do . .	1	0·	0·0280287		
Do♯ re♭	1·066′	0·0280287		0·0231238	
Re . .	1·125	0·0511525	0·0280287		
Re♯ mi♭	1·2	0·0791812			0·0177288
Mi . .	1·25	0·0969100	0·0280287		
Fa . .	1·33′	0·1249387		0·0231238	
Fa♯ sol♭	1·40625	0·1480626	0·0280287		
Sol . .	1·5	0·1760913	0·0280287		
Sol♯ la♭	1·6	0·2041200			0·0177287
La . .	1·66′	0·2218487	0·0280287		
La♯ si♭	1·77′	0·2498775		0·0231238	
Si . .	1·875	0·2730013	0·0280287		
Do_2 . .	2	0·3010300			

While the departure from a geometrical progression is in this case somewhat greater than in the Pythagorean scale, there is here more simplicity in the relation which the vibrations of each note bear to those of the tonic, whence probably its greater harmonic power. There is another result following from the departure from a regular geometrical progression, both in the case of the Pythagorean and the harmonic scale. According to the ideal scale, in which the geometrical progression is regular, all major keys would have been exactly similar, and so would all minor keys. But the departures from regularity make every one major key to differ from every other major key, and so also with the minor, thus affording a much greater variety.

Now, as regards the correspondence of the scale of colour with one or other of the musical scales, it was at one time thought to be closer than it really is. For the rates of vibration corresponding to the junction of the colours were believed to constitute the following series,—1, 1·125, 1·2, 1·33′, 1·5, 1·66′, 1·77′, 2,—thus tallying with the harmonic scale in its minor mode. Prof. Listing, however, by a careful comparison of the most recent and accurate observations,—those made by Angström and others,—has determined, with a greater approximation to the truth, the wave-lengths corresponding to the borders of the several colours, and has shown that the reciprocals of those wave-lengths, which correspond to the ratios of the vibrations at those points, form a series approaching much more closely to an arithmetical than to a geometrical progression. (See Pog. An., vol. cxxxi., p. 564).

When the reciprocals of Prof. Listing's wave-lengths have their relations reduced to the simplest form, by making the smallest number = unity, they form the following series:—1, 1·117738, 1·235314, 1·352908, 1·470618, 1·588145, 1·705730, 1·823568.

There is here an evident approach to a common difference, of which the mean value is 0·117653.

This approach to an arithmetical progression becomes more apparent when the arithmetical means of the reciprocals of Listing's numbers are taken. These form the following series:—

Red.	Orange.	Yellow.	Green.	Blue.	Indigo.	Violet.
1,	1·111064,	1·222108,	1·333213,	1·444288,	1·555303,	1·666453.

It is evident what is the true law of this series, namely, that all of the above numbers should be perfect repeating decimals, having a common difference of 0·11′. This series, thus corrected, being assumed, it is easy to calculate backwards, so as to show the agreement of this assumption with observation. Taking the green as the central colour, by applying the above corrected series, we obtain from each of the other colours a value of the green; and the average of these six values will be found to differ by a mere trifle from the value deduced from the observations. From this corrected value of the green all the others may be found by the above series, and the resulting values of the reciprocals of the wave-lengths for the mean colours will stand thus:—

Red.	Orange.	Yellow.	Green.	Blue.	Indigo.	Violet.
1463·676,	1626·307,	1788·937,	1951·568,	2114·198,	2276·829,	2439·460,

forming an arithmetical progression, of which the common difference is 162·631.

From the above, result the following numbers for the borders of the colours:—

1382·360, 1544·991, 1707·622, 1870·252, 2032·883, 2195·513, 2358·144, 2520·775,

—an arithmetical progression, of which the common difference is also 162·631.

From this last series we obtain the wave-lengths of the borders of the colours, in order to compare them with the wave-lengths given by Listing from observation. The following table gives the result:—

Observed.	Calculated.	Differences +	Differences −
7234	7234·09	0·09	
6472	6472·529	0·529	
5856	5856·1	0·1	
5347	5346·873		0·127
4919	4919·121	0·121	
4555	4554·744		0·256
4241	4240·624		0·376
3967	3967·034	0·034	
		0·874	0·759

These small differences are considerably within the limits of probable errors of observation, the more especially as Listing's numbers do not extend beyond four figures. It may accordingly be fairly concluded that, when reduced to their simplest form, by making the lowest number unity, the series will be for the borders of the colours—1, 1·117647, 1·235295, 1·352942, 1·470589, 1·588236, 1·705883, 1·832530; and for the mean rays of the colours—

Red.	Orange.	Yellow.	Green.	Blue.	Indigo.	Violet.
1,	1·11′,	1·22′,	1·33′,	1·44′,	1·55′,	1·66′

On comparing these two series with the three musical scales, it will be perceived that with these the first series has no points of correspondence whatever. Neither has the second series with the ideal musical scale. But in the two others the green corresponds exactly with the *fa*, while the violet tallies also with the *la* of the harmonic scale only. Moreover, if the violet be divided by the orange, the quotient, 1·5, will correspond to the *sol* of both scales. So, also, if the green be divided by the orange, the quotient 1·2, is equal to *mi*♭, or the minor third of the harmonic scale; while the violet divided by the green gives 1·25, corresponding to its major third.

Beyond these points of correspondence, in themselves not a little remarkable, there is no analogy between the scale of colour and the musical scales. The analogy is closest in the case of the harmonic scale; but there is this fundamental difference, that, whereas there are in that scale three interlaced arithmetical progressions, with diverse common differences, the colour scale consists of a single perfect arithmetical progression; so that, in their integrity, the two scales are irreconcilable. It is thus evident that the analogy between the two scales, so far from being perfect, consists only in this, that both are founded on a mathematical basis; but the colour scale forms a series much more simple and symmetrical than does either the Pythagorean or the harmonic musical scale.

These mathematical relations, subsisting among the mean rays of each pure colour of the spectrum, become all the more interesting when viewed in connection with those subsisting among the principal fixed lines of the normal spectrum, as respects their relative wave-lengths. For the purpose of a comparison of the one set of relations with the other, the latter may here be given as deduced from the very accurate observations of M. Angström. Assuming the more refrangible E as the centre of the system, and calling the value of its wave-length 10, the relative wave-lengths corresponding to the other fixed lines A, B, C, the less refrangible D, F, G, and the more refrangible H, may be found by the following formulæ:—

$$\begin{array}{lll}
(a) & 2A^2-6A \quad . \quad . \quad . \quad . \quad . \quad . & = 3E^2+3E \\
(b) & (6A^2-A)-(6B^2+2B) \quad . \quad . & = E^2+9E \\
(c) & (4B^2+2B)-(4C^2-2C) \quad . \quad . & = E^2+E \\
(d) & (6C^2-C)-(6D^2+6D \quad . \quad . & = E^2 \\
(f) & C^2+(2F^2-6F) \quad . \quad . \quad . \quad . & = 2E^2+7E \\
(g) & (4G^2+4G)-(C^2+2C) \quad . \quad . & = E^2+2E \\
(h) & (2F^2+6F)-(H^2+4H) \quad . \quad . & = E^2+4E
\end{array}$$

The relative values of the eight wave-lengths, as given by observation, and as calculated from the foregoing equations, are as follows:—

	Observed.	Calculated.	Differences +.	Differences −.
A.	14·432490	14·432517	0·000027	
B.	13·033840	13·033839		0·000001
C.	12·454950	12·454930		0·000020
D.	11·189030	11·189003		0·000027
E.	10			
F.	9·225744	9·225760	0·000016	
G.	8·175214	8·175183		0·000031
H.	7·464880	7·464871		0·000009
			0·000043	0·000088

These trifling differences are much within the limits of probable errors of observation.

The foregoing seven equations give rise to the following more general one, embracing all the wave-lengths:—

$$a+b+c=d+f+g+h=6E^2+3E=630.$$

They also produce the following series:—

$$d = 100$$
$$a+h-f = 200$$
$$b+c = 300$$
$$a+b-g = 400$$
$$c+f+g = 500$$
$$a+f = 600$$
$$a+c+g+h = 700$$
$$2a+h = 800$$

These relations, taken in connection with the agreement between the wave-lengths calculated from the equations and those obtained from observation, render it in the highest degree probable that they have a true mathematical basis. They show that these wave-lengths are interdependent; so that no alteration can be made on any one of them without involving a corresponding change in all the rest.

Here the question arises—Do the intervals between any of those fixed lines among themselves, or between them and any other well known lines in the spectrum, correspond to any of the musical intervals, so as to render it probable that they are harmonically related? The first case that presents itself for consideration is that of hydrogen, in the spectrum of which occur two of the principal fixed lines, C and F, besides two other lines, designated as Hy_3 and Hy_4. As already mentioned, the wave-lengths of the three lines C, F, and Hy_4 stand to each other approximately in the ratio 20, 27, 32. Now as respects F and C, the foregoing formulæ may be applied to ascertain the accuracy of this relation between those two lines. Do they stand in the precise ratio 20 : 27 or 1 : 1·35? Taking the observed wave-lengths as given by Angström, the ratio is 1 : 1·350206. Taking the wave-lengths calculated from the formulæ, the ratio is

1 : 1·35017. But while in the first case the discrepancy might be attributed to errors of observation, it cannot be so attributed in the case of the calculated wave-lengths: for these could not be altered, even to so trifling an extent as to make the ratio exactly 1 : 1·35, without destroying the whole symmetry of the formulæ. It is accordingly far more probable that the ratio between F and C is as 1 : 1·35017 than exactly 1 : 1·35—a relation which would involve the conclusion that all the foregoing nicely balanced formulæ have no true mathematical basis, but arise out of mere arithmetical coincidences. Moreover, the ratio 1 : 1·35 does not correspond to that of any musical interval. The case is different with the relation between F and Hy_4, which is so nearly that of a Pythagorean minor third that the difference might be ascribed to errors of observation. If the calculated value of F be divided by the ratio of this minor third 32÷27, it will give for the wave-length of Hy_4 4101·258; whereas Angström makes the observed value 4101·2—the difference 0·058 lying much within the limits of probable error, so that the true relation between F and Hy_4 may very probably be that of this minor third. But it is the minor third proper to melody—not that proper to harmony, the ratio of which is 6÷5.

With respect to the other principal fixed lines, generally,—those namely embraced in the foregoing formulæ,—it may be affirmed that none of them stand to each other in a ratio corresponding to any of those found in the three several musical scales. Nevertheless, each of the lines stands in a relation of that kind to one or more other lines of the spectrum, within the probable limits of error of observation. These relations are shown in the annexed table, of which the first column contains the letter designating the fixed line; the second, the sign of multiplication or division. The next three columns the name of the musical interval in one or other of the three scales—the Ideal, the Pythagorean, the Harmonic, by which the wave-length of the fixed line is multiplied or divided. The sixth column contains the wave-length resulting from this multiplication or division. The seventh contains the corresponding wave-length in Angström's scale, to which it is nearest. The eighth shows the differences, plus or minus, between the two—these all lying within the limits of probable errors of observation. The ninth contains the names of the elements to which the wave-lengths are respectively due, and the tenth the colour of the region of the spectrum in which each wave-length occurs.

Looking to the general character of the relations exhibited in the table, they do not appear to encourage the supposition of their indicating that the lines thus connected correspond to rates of vibration, having their origin harmonically in one common vibration. The most obvious and simple interpretation of them is, that the ratios are those of the respective amounts of *vis inertiæ* possessed by the vibrating atoms which originate the lines; while their arithmetical coincidence with certain musical intervals is merely accidental, and such as might be expected, according to the law of probabilities, where so large a number of lines are concerned.

If diverse rates of vibration, having their origin harmonically in a common rate of vibration, might be looked for anywhere, it is in the lines produced by the same element. Yet such lines are not, as a general rule, thus harmonically related. The principal fixed lines E and G are both iron lines; but there is no harmonic connection between them, although E appears to be harmonically related to another iron line in the indigo, and G to another in the green. But the number of iron lines is so great that these may well be mere arithmetical coincidences. If we take another example, such as magnesium, in which the lines are few and conspicuous, we shall find that their ratios do not correspond to any musical interval. These magnesium lines are four in number, and their wave-lengths, according to Angström's scale, are 5527·54 in the yellow, 5183·10, 5172·16, and 5166·88—all three in the green. The ratios subsisting between any two of these are too small to be harmonic. The ratio between the first and last, though greater than a semitone, is less than a tone. Between the first and third the ratio approaches near to that of *do* to *re*♭ in the Pythagorean scale; but this interval is highly discordant.

On the whole, therefore, whether we take the mean colours of the spectrum, the principal fixed lines, or the lines produced by any single substance, it cannot be affirmed that there is between colours and musical tones any analogy, beyond that of their being both produced by vibrations; while the relations of those vibrations are in each case governed by mathematical laws. But these laws are in the case of colours much more simple and regular than in the case of musical sounds, in which they are discontinuous, irregular, and complex.

The points of diversity between the two sorts of vibrations are also very marked. The normal eye can judge much more promptly and correctly of a simple colour than can the normal ear of a simple musical note. In colour there is always involved the idea of superficies; and although time is also really involved, yet the rapidity of vibration is such that the mind can form no conception of it whatever. In music, on the other hand, there is not involved any idea of superficies; whereas time is an indispensable element in the conception of a musical note. Again, there is in music nothing corresponding to complementary colours or to the perception of mere whiteness, which can be produced by a combination of such.

Harmony in music is produced by the simultaneous impulse on the ear of two or more combined sounds, whose rates of vibration stand to each other in certain definite arithmetical relations; and whenever there is any departure from those relations the result is either dissonance or discord. When two or more colours fall simultaneously on the same point of the retina, the result is a compound colour, which may or may not be pleasing to the eye; but the mixture of adjacent colours in the spectrum is not displeasing to the eye, as would be the simultaneous sounding of two adjacent musical notes to the ear. What is called harmony in colour depends, not on the simultaneous impulse of two or more waves of colour on one and the same point of the retina, but on the juxtaposition of two or more colours without admixture. The effect seems to depend on the definite arithmetical relations which the rates of vibration corresponding to the colours bear to each other, as in the case of sound.

These effects of the juxtaposition of colours, however, are much more analogous to melody in music than to harmony. The pleasing impression, for example, produced by the gradual blending of the adjacent colours in the refracted spectrum is analogous to the slide in the violin. The juxtaposition of pure orange and pure violet doubtless owes its agreeable impression to the circumstance that these two colours stand to each other in the same ratio as do the *do* and *sol* of the musical scale. In like manner the effect produced by the juxtaposition of pure red and pure green is due to their standing to each other in the same ratio as *do* and *fa*. So also with pure red and pure violet, which bear the same mutual relation as *do* and *la*, or pure green and pure violet, which are related as *do* and *mi;* likewise pure orange and pure green, which are related as *do* and *mi*♭. But in all these cases the effect is more analogous to that produced by the striking of these notes in rapid succession as parts of a melody, than to the harmony resulting from

Table of Spectral Lines and Musical Intervals.

Line.	Sign.	Ideal.	Pythag.	Harmonic.	W. Length.	W. L. Ang.	Difference.	Elements.	Colours.
A.	÷	—	Sol♭	—	5340·54	5339·35	1·19	Fe.	Gr.
	÷	Fa♯	—	—	4790·23	4791·78	1·58	Co. Ti.	Bl.
	÷	—	—	La♭	4752·51	4753·47	0·96	Mn.	,,
	÷	La	—	—	4521·38	4522·09	0·71	Ti.	In.
	÷	Si	—	—	4028·1	4029·5	1·4	Mn.	Vi.
	÷	—	Si	—	4005·4	4004·9	0·5	Fe.	,,
B.	÷	—	—	Re♭	6437·91	6438·35	0·44	Ca.	Or.
	÷	—	Re♭	—	6430·64	6430·12	0·52	Fe.	,,
	÷	—	Sol♭	—	4822·98	4822·9	0·08	Mn.	Bl.
	÷	Sol♯	—	—	4326·0	4325·34	0·66	Fe.	In.
C.	÷	—	Mi♭	—	5462·24	5462·44	0·2	Fe.	Ye.
	÷	Mi	—	—	5208·34	5207·78	0·56	Cr.	Gr.
	÷	—	Mi	—	5184·86	5183·1	1·76	Mg.	,,
	÷	—	—	Sol♭	4666·37	4666·45	0·08	Fe. Ti.	Bl.
	÷	—	Sol♯	—	4152·57	4153·79	1·22	Fe.	Vi.
	÷	Sol♯	—	—	4133·86	4133·94	0·08	Fe.	,,
D.	×	Re♭	—	—	6245·66	6245·62	0·04	Fe.	Or.
	÷	—	—	Re♭	5526·60	5527·54	0·94	Mg.	Ye.
	÷	Mi♭	—	—	4957·28	4956·87	0·41	Fe.	Gr.
	÷	—	Fa♯	—	4196·82	4197·98	1·16	Fe.	Vi.
	÷	—	—	Sol♭	4192·0	4191·17	0·83	Fe.	,,
E.	×	Mi	—	—	6265·54	6264·31	1·23	Fe.	Or.
	×	—	Re♯	—	6244·35	6245·62	1·27	Fe.	,,
	×	Mi♭	—	—	5913·88	5913·3	0·58	Fe.	,,
	÷	—	Re♭	—	4933·8	4933·55	0·25	Fe. Ba.	Gr.
	÷	—	—	Mi♭	4492·83	4493·81	0·98	Fe.	In.
F.	×	—	Re♯	—	5760·89	5762·04	1·15	Fe.	Ye.
	×	Re	—	—	5456·0	5454·84	1·16	Fe.	,,
	×	—	—	Re♭	5184·8	5183·1	1·7	Mg.	Gr.
	÷	—	Re♯	—	4101·26	4101·2	0·06	Hy.	Vi.
G.	×	—	Sol	Sol	6460·85	6461·98	1·13	Ca.	Or.
	×	—	—	Mi♭	5168·68	5168·48	0·2	Ni. Fe.	Gr.
	÷	—	Re♭	—	4033·47	4032·9	0·57	Mn.	Vi.
H.	×	Mi	—	—	4955·27	4956·87	1·6	Fe.	Gr.
	×	—	Re	Re	4424·63	4425·07	0·44	Ca.	In.
	×	Re	—	—	4414·64	4414·77	0·13	Fe. Mn.	,,
	×	—	Re♭	—	4199·95	4201·56	1·61	Fe.	Vi.
	×	—	Do♯	—	4143·4	4143·14	0·26	Fe.	Vi.

their being struck simultaneously, the analogy to which can arise only from the perfect admixture of colours.

The effect produced on the eye by the juxtaposition of complementary colours, again, seems to depend on a different principle from that of the rates of vibration standing to each other in a musical ratio. It results solely from the circumstance that the admixture of the two adjacent colours would give white; and as three colours are required to produce this effect, one or both of the adjacent complementary colours must contain the necessary third colour in the proper proportion required to constitute white. The retina experiences a pleasurable relief on turning from the one complementary colour to the other, because the vibrations are then most opposed. Here, also, the effect is more analogous to melody than harmony. The ear experiences the same weariness from the prolongation of one note as the eye does from gazing on one colour; and as the latter feels the greatest amount of relief when turned to the complementary colour, so the ear feels the greatest amount of relief, when, after being fatigued with one note, it hears another which would make with it a harmonious combination, as the third, fifth, or octave.

It is, however, in their metaphysical qualities that the two sorts of vibrations most widely differ; and here the advantage rests with the musical tones. Apart from variety of form, colours can be regarded only as more or less pleasing or the reverse. Beyond this they have little or no emotional power. Music, on the other hand, addressing the imagination, can express, awaken, or exalt every emotion of the mind. It is only when united to variety of form that colours acquire the ascendency over their sonorous rivals. Then, indeed, they become by much the more powerful vehicle for conveying ideas, whether intellectual or emotional, to all but the blind.

VI. REMARKS UPON THE PRESENT STATE OF THE DEVONIAN QUESTION.

By Horace B. Woodward, F.G.S.,
Geological Survey of England and Wales.

ONE of the most interesting questions that has of late years perplexed the minds of geologists, and one which we might almost say has been a vexed point ever since the district was studied, is the age and relations of the slaty rocks and limestones of West Somerset, Devon, and Cornwall. Originally called "Greywacke" and transition slates, the beds below the culm-measures or true coal-measures were subsequently called "Devonian," and regarded as the marine equivalent of the old red sandstone. Latterly this classification has been called into question, and it has been urged that the greater part of the Devonian rocks are of lower carboniferous age. This last opinion being a matter of great dispute, it may be interesting to review the present state of the question.

Upon glancing at a geological map of the country, such as Greenough's, we find that part of Devon north of Barnstaple and South Molton, and that part of West Somerset which includes Exmoor Forest, the Brendon and Quantock Hills, to be coloured a uniform tint as Devonian, corresponding to that of the old red sandstone of the Mendips, South Wales, and Herefordshire. This area of the Devonian rocks is bounded on the south by the culm-measures or carbonaceous series, and the boundary line between the two formations is marked on either side by narrow and apparently impersistent bands of limestone, which, to judge from the map alone, would appear to bind them together in conformability.

Until the late Mr. Jukes brought forward his views upon the subject, the age of these formations was generally looked upon in this way—that the Devonian rocks represented in time the old red sandstone, and that the culm-measures were of carboniferous age, newer than the mountain limestone. In Greenough's map these latter were coloured as the representation of the millstone grit, and the same is the case in Ramsay's geological map of England and Wales.

This apparent indecision as to the true age of the culm-measures has necessarily caused much obscurity when the relations of the two series, and their generally acknowledged conformability, have been taken into consideration.

Looking to the origin of the term "Devonian," we learn that Mr. Lonsdale was the first to point out that the characters of the fossils of the Devon limestones seemed to give them an intermediate place between the upper silurian rocks and the mountain limestone, and it was this suggestion which in 1839 led Sedgwick and Murchison* to adopt the term "Devonian system" for the series of rocks in North and South Devon which underlie the culm-measures. Henceforth they were regarded as contemporaneous with the old red sandstone. It was even then hinted that possibly the mountain limestone was represented by a part of the culm-measures, and when one refers to the subsequent papers one sees how much room there was to doubt the clearness of this correlation, and in the writings of De la Beche particularly, we find the difficulties attending it fully pointed out.† This is apparent when he compares the Upper Devonian rocks with the upper portion of the old red sandstone, as exhibited at no very great distance apart. For the upper beds of the old red sandstone in South Wales and the Mendip Hills show no similarity whatever to the Upper Devonian rocks. He, moreover, refers to the view taken by Mr. (now Sir Richard) Griffith in 1842, who then pointed out the strong resemblance between the North Devon rocks and those beds in Ireland, which he called carboniferous slate and yellow sandstone, deposits equivalent to the transition beds, or lower limestone shale, between the old red sandstone and the mountain limestone.‡ These views, in fact, were almost the same as those at which Mr. Jukes arrived.§ It was in 1866 that his famous paper was read before the Geological Society of London,‖ and therein Mr. Jukes brought forward (though not for the first time) the views, which for fifteen years previously he had been thinking over, and which led him to consider the rocks of North Devon to belong partly to the group called carboniferous slate in Ireland, and partly to the old red sandstone. He based his interpretation upon an intimate knowledge of the geology of the South of Ireland, where he found that the mountain limestone which was separated from the old red sandstone by the carboniferous slate became in places entirely replaced by the slate, so that this slate then filled up the whole of the interval between the top of the old red sandstone and the base of the coal-measures. Here he contended was the clue to determine the structure of North Devon; the order of sequence appeared to him the same in both localities, so that the so-called Devonian rocks were really the lower portion of the carboniferous system, resting, as in Ireland, upon a base of the old red sandstone.*

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* Trans. Geol. Soc., 2nd Ser., vol. v., p. 633.

† Mem. Geol. Survey, vol. i., p. 65.

‡ *Idem*, p. 76.

§ Additional Notes on the Grouping of the Rocks of North Devon and West Somerset. 8vo. Dublin, 1867. P. 19.

‖ Quart. Journ. Geol. Soc., vol. xxii., p. 320.

In 1867 Mr. Jukes published a small map, which more clearly expressed his ideas. The true old red sandstone he considered to occur at the North Foreland, Minehead, and Croydon Hill, also at the north-western end of the Quantock Hills; then succeeded the carboniferous slate at Lynton, Combe-Martin, Ilfracombe, Mortehoe, and the Brendon Hills; while stretching from Pickwell Down to Haddon Down, Mr. Jukes identified another band of old red sandstone, to account for which he considered that a great fault, with a downthrow to the north, occurred along this line and repeated the beds to the south,—the carboniferous slate coming conformably over this band of old red sandstone, and then again passing gradually into the culm-measures above.

Mr. Jukes's views met with great opposition at the time, but as few, if any, of his opponents had a personal knowledge of the geology of the South of Ireland, they could not perhaps fully realise all the facts which guided him in his inferences.

Mr. Etheridge,† however, took up the question in great detail, and though perhaps he laid greatest stress upon the palæontological evidence, he yet disputed the conclusions of Mr. Jukes on physical and stratigraphical grounds, and maintained that there was no evidence of any fault, as the succession of the strata and the groups of associated fossils from the North Foreland to Barnstaple was continuous and natural. The area he considered to be occupied by three well-defined groups—the Upper, Middle, and Lower Devonian, chronologically equivalent to the whole of the old red sandstone, but deposited under different mineral and life conditions, and in a *different geographical area*. The fossil evidence, in his opinion, was against any repetition of the beds, and nowhere justified the proposition that the Devonian beds were synchronous with the carboniferous. In discussing this question, however, Mr. Jukes argued that the difference between the fossils from different parts of the so-called Devonian rocks did not differ more markedly from each other than fossils from different parts of the carboniferous slate differed from each other; that the fossils of both groups warranted the conclusion that they might have been geologically contemporaneous.*

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* *Vide* JUKES and GEIKIE, Manual of Geology, 1872, p. 762.

† Quart. Journ. Geol. Soc., vol. xxiii., p. 568.

The palæontological evidence cannot, therefore, be looked upon as decisive. The Devonian beds contain some species which are also found in silurian rocks, and many species that occur in the mountain limestone. The old red sandstone does not contain any of these fossils; "there are *no* marine forms in the old red sandstone."† Certain fish remains have, however, been found by Mr. Pengelly‡ in the Devonian rocks of Cornwall, which are also found in the old red sandstone. These include *Pteraspis* and *Phyllolepis concentricus*. This, Mr. Jukes remarks, is the strongest presumptive evidence yet derived from fossils in favour of the contemporaneity of the two formations. Nevertheless, he adds, it is not conclusive proof, for it is obvious that the occurrence in the Devonian rocks of species of fossil fish belonging to the same genera as those of the old red sandstone no more proves the Devonian beds to have been contemporaneous with the old red sandstone, than the occurrence of species of trilobites, of the same genera as those in the Silurian rocks, prove the Devonian rocks to be contemporaneous with the Silurian.§ Therefore, we must agree with Jukes that the geological age of the fossils must be proved by the stratigraphical position of the beds.

In setting forth the present state of the question, two points connected with the subject, which have recently been brought forward, may be referred to.

An interesting feature has been noticed by Mr. T. M. Hall in connection with the granites of Lundy Island, South Devon, and Hestercombe, near Taunton (first described by Mr. Leonard Horner‖). He remarks that, although the most remote of the three patches, "the so-called granite (syenite) of this last locality has been regarded as possessing a more intimate connection with Lundy Island, since the general run of the Palæozoic rocks in North Devon and the adjoining portion of West Somerset is from east to west; and it might, therefore, be suggested that least resistance would be afforded to the intrusion of an igneous rock at the various places situated along the same line of strike."[*]

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* Jukes and Geikie, Manual of Geology, 1872, p. 763.

† Etheridge, *op. cit.*, p. 679.

‡ Mr. Pengelly stated that he had found 300 specimens of Pteraspidian fishes in the Devonian rocks. Brit. Assoc. Meeting, Exeter, 1869.

§ Jukes, Notes on Parts of South Devon and Cornwall, p. 42.

‖ Trans. Geol. Soc., vol. iii., p. 348.

The chief point of interest connected with these observations is the bearing they may have on the supposed fault of Mr. Jukes, a consideration which in reference to the little syenitic dyke at Hestercombe was suggested by Mr. Bristow, when on a visit to this spot on geological survey work.

The age of the Cannington Park limestone has been a constant source of discussion, and still the opinions vary. Mr. Etheridge[†] spoke in decided terms of its Devonian characters, and of its dissimilarity to the mountain limestone; while more recently its identity with this latter formation has been again insisted upon.[‡] The scarcity of fossils has somewhat hindered its true position being established: but Mr. S. G. Perceval[§] has lately examined a series of corals collected there, and which he finds to be of true carboniferous genera and species. The structure of the limestone he also identifies with that of the mountain limestone of the neighbourhood of Bristol. The very diversity of opinion on this patch of limestone would seem to mark it as a connecting link between the mountain and Devonian limestones, and so to lend support to Jukes's view that both belong to the same period.

Thirty years ago De la Beche remarked, that "from the increased knowledge we have lately had of the beds which may be considered as the passage of the old red sandstone into the carboniferous limestone, as well in Ireland as in South Wales, and in adjacent parts of England, we have endeavoured to point out, as not improbable, that in North Devon some part, at least, of the accumulations there exposed might be referable to that date." He also observes that "there is much leading us to infer that in South Devon the accumulations under notice were not far removed from a similar geological date."[||] Mr. Etheridge admits that there may be grounds for endeavouring to establish contemporaneity between the Upper Devonian series of North Devon and the carboniferous slates of the South of Ireland.[¶]

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[*] T. M. Hall, Notes on the Geology and Mineralogy of the Island of Lundy. Trans. Devon Assoc., 1871.

[†] Quart. Journ. Geol. Sec., vol. xxiii., p. 581.

[‡] H. W. Bristow and H. B. W., Geol. Mag., vol. viii., p. 504.

[§] Geol. Mag., vol. ix., 1872, p. 94.

[||] Mem. Geol. Survey, vol. i., p. 97.

[¶] *Op. cit.* p. 690.

The precise age of the culm-measures and their relations to the Devonian rocks are points which at first strike one as of great importance.

The perfect conformability of the northern boundary of the culm-measures with the Devonian rocks has generally been admitted, by Sedgwick and Murchison, and most other geologists.

In South Devon, however, an unconformability has been pointed between these formations. This was described by Mr. Godwin-Austen, and latterly by Dr. Holl, who remarks that the base of the lower culm-measures does not everywhere rest on the same part of the underlying Devonian rocks. He adds, that "this unconformability on the southern side of the culm-trough is so considerable that it throws doubt upon the reality of the apparent regular succession to the north, and leads to the suspicion that the conformability which is there supposed to exist may be more apparent than real."*

Mr. Jukes,† however, points out that there is really no proof of this unconformability in South Devon, owing to the difficulty in deciding between stratification and cleavage, and the many disturbances to which the beds have been subjected.

Mr. T. M. Hall, remarking on the Devonian and culm-measures, says—"The two great systems pass quite insensibly one into the other, without any distinct line of separation between them."‡ And this is evident from the sections exposed in quarries and in the cuttings of the new railway between Barnstaple and Taunton, for one passes from one series to the other before one is aware of it; there is no sudden break or change.

The age of the culm-measures is now admitted to be that of our true coal-measures. For in the evidence given before the Royal Coal Commission there was some question as to whether the coal-measures likely to be found to the south of the Mendips might not be of the type of the Devonian culm-measures; and Mr. Etheridge also said that he was inclined to think that the Devonshire coal-field was part and parcel of the South Wales coal-field, the lowest portion of it, but deposited under very different conditions,—an opinion which was indeed arrived at by Sedgwick and Murchison. He thought that the impure coals of the

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* Quart. Journ. Geol. Soc., vol. xxiv., p. 442.

† Notes on Parts of South Devon and Cornwall.

‡ Geology of Lundy Island.

Millstone Grit series were the equivalents of those beds which lie south of Barnstaple.*

Mr. Jukes identified the culm-measures as exactly like the Irish coal-measures, especially in the Kilkenny coal-field.†

From this it naturally follows that the beds beneath the culm-measures must represent the lower carboniferous rocks, and in part, at any rate, Mr. Jukes's notions must be correct. He would limit the term Devonian, and retain its use, for those beds containing the marine fossils commonly known under the name of Devonian fossils. The old red sandstone does not contain any of these fossils, and is a group of rocks distinct and altogether below them. He further ventured to advance the notion that the Devonian beds may rather be looked upon as the most general type of those which intervene between the coal-measures and the old red sandstone, and that the mountain limestone is rather a local and exceptional peculiarity.‡

On the other hand, Mr. Etheridge considers that we must either admit that the Devonian is a marine equivalent in time of the old red sandstone, or that it must be a distinct life-system, occupying an immense area, spreading over an enormous interval of time between the completion of the old red sandstone as a whole and the commencement of the succeeding and well-marked carboniferous series.§ The latter opinion seems to be that generally adopted; for in remarking upon the opinions since expressed, if we do not find a tendency towards the acceptance of Mr. Jukes's views, we see that geologists are beginning to regard the Devonian rocks as newer than the old red sandstone.

Mr. Godwin-Austen has stated that he had always regarded the Devonian system as merely an older member of the Carboniferous, holding much the same relation to it as the Neocomian to the Cretaceous; and that he would be glad to see it recognised, not as an independent system, but merely as the introduction of that far more important system the Carboniferous, during the deposit of both of which the globe presented the same physiographical conditions.‖

Professor Phillips, too, observes that "the old red sandstone is followed in Devonshire, and still more remarkably in the South of Ireland, by a series of shales, grits, and limestones, with a large *suite* of fossils, having on the whole a considerable analogy with the still richer associations of marine life in the carboniferous limestone. . . . Near Linton, in North Devon, and south of Plymouth, we may satisfy ourselves of the fact that old red sandstone underlies the Devonian beds. . . . From this series of rocks to the carboniferous strata which succeed the transition is easy,—so easy indeed that, in the opinion of Sir R. Griffith and Mr. Jukes, the whole of the Devonian series may be united with the lowest members of the Irish carboniferous group (yellow sandstone and carboniferous slate). What seems ascertained truth is the close approximation in time, in character of deposition, and in forms of life, of the South Hibernian and South Welsh rocks; while the North Devonian strata contain with these a somewhat lower group, not distinctly represented in Wales or Ireland."*

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* Report of Coal Commission, vol. ii., p. 421.

† Notes, &c., p. 31.

‡ Quart. Journ. Geol. Soc., vol. xxii., p. 369.

§ *Ibid.*, vol. xxiii., p. 613.

‖ *Ibid.*, vol. xxviii., 1872, p. 30.

Whether we regard the Devonian slates as the equivalents of the old red sandstone or of the lower carboniferous rocks, a great change in sedimentary condition must have taken place; and the question is still perhaps to be decided, whether part of the Devonian rocks are a modified extension of the old red sandstone—a point which appears to take its stand merely on palæontological evidence—or whether the whole of the fossiliferous Devonian slates and limestones be not of lower carboniferous age, the representatives of the mountain limestone and the lower limestone shale, and of the carboniferous slate and limestone of Ireland. This latter opinion finds the more support when we look, as Mr. Jukes and others have pointed out, to the variations which take place in the carboniferous limestone series when traced through the north of England into Scotland, as well as through the South of Ireland.

Looking at the culm-measures as representing the true coal-measures, and perhaps also the millstone grit, and that they pass gradually downwards into the Devonian rocks, we may possibly find, in the numerous thin bands of limestone which occur along the junction, some feeble representation of the upper part of the mountain limestone; then come a series of slates, which must in part represent the mountain limestone, the whole of the lower limestone shale, carboniferous slate, and perhaps a part of the old red sandstone. Beneath these come beds of the acknowledged type of the old red sandstone.

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* Geology of Oxford and the Valley of the Thames, p. 79.

At any rate, in this conformable series we have to look for the equivalents of the lower carboniferous series. It may not be possible to fix any of the divisions, as we find them marked at no very great distance away, in South Wales and in the Mendip Hills; but in these places it is often difficult enough to fix a precise line, so gradually do they merge one into the other, though they are clear enough when looked at in a large way. A greater similarity of conditions prevailed over the Devonian area, and naturally the fossils differ from those found elsewhere in varying sedimentary deposits of the same period.

Whether the supposed fault of Prof. Jukes can be proved or not is a matter that it is difficult to foresee. Possibly the new line of railway in course of construction between Barnstaple and Ilfracombe may afford some decisive evidence. Let us hope, at any rate, that it may yield many good sections. At present, as Mr. Jukes remarks, whilst there is no direct evidence of the fault, yet no certain physical or stratigraphical evidence has been adduced against it.

That there is much to be done in this field is a point about which no doubt can exist. The workers have been many; and the names of Sedgwick, Murchison, Lonsdale, De la Beche, Godwin-Austen, Phillips, Jukes, and Etheridge, must always command the highest respect of the followers in the same field.* They have all done great work in elucidating the structure of a difficult country; and as their followers have the advantage of their labours, so the path becomes easier, and whenever a final solution of the question is arrived at, it will probably be by a transition in opinion as easy as that which binds the series of rocks together.

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* A list of works on the Geology of Devonshire has been compiled by Mr. Whitaker. See Trans. Devon. Assoc., vol. iv., p. 330, and vol. v., p. 404.

NOTICES OF BOOKS.

The Expression of the Emotions in Man and Animals. By CHARLES DARWIN, M.A., F.R.S., &c. London: Murray. 1872.

AN insatiable longing to discover the causes of the varied and complex phenomena presented by living things seems to be the prominent characteristic of Mr. Darwin's mind. Nothing is so insignificant as to escape his notice or so common as not to demand of him an explanation. The restless curiosity of the child to know the "what for," the "why," and the "how" of everything (a wholesome curiosity which our educational system represses, and which rarely survives to manhood) seems with him never to have abated its force; but he is by no means satisfied, as the child is, with mere verbal explanations which really explain nothing, or, as many writers on this particular subject have been, with purely speculative explanations which are wholly unsupported by evidence.

The present work exhibits these characteristics of the author's mind in an eminent degree, since we here find systematised and explained by means of acknowledged physiological and psychological facts all the immense variety of complex movements and minute muscular contractions, by the observation of which we unconsciously interpret, with more or less certainty, the almost infinitely varied passions and emotions of men and animals. How few of us have ever thought of asking for a reason why infants shut their eyes tightly while screaming; why we shrug our shoulders or stand erect, blush or grow pale under different emotions; why a dog crouches and a cat arches its back when affectionate; or have even imagined that satisfactory reasons for these things could be given? Yet we can hardly help being interested in so novel an enquiry, and one which throws so much light on actions and movements which constitute a kind of universal language, but which have hitherto appeared arbitrary and inexplicable to us.

The result of Mr. Darwin's study of this subject is the establishment of three general principles, which explain and give a meaning to almost all those involuntary gestures and movements by which men and animals express their emotions. The first of these principles is that of Serviceable Associated Habits. When any action has been useful or necessary under a certain state of mind, it will from association continue to be performed whenever the same state of mind recurs, even if of no use. As an instance we may take the case of dogs turning round several times before they lay down to sleep even on a carpet or floor, and sometimes giving a few scratches, a practice which was no doubt useful when the wild animal slept among herbage out of doors, and which is continued now as a habit when of no such use. The second is the principle of Antithesis, which is, that certain actions or attitudes being the natural acompaniment of a given emotion or state of mind, the opposite state of mind will be expressed by actions or attitudes which are, as far as possible, the exact opposites of the former. A good example of this is given by the case of the dog and cat. The former crouches down and holds down its tail when licking its master's hands or jumping on his knees; but the cat while rubbing against its master's leg, stands erect with somewhat arched back and tail up on end. These attitudes are explained by their being in each case the opposite of those assumed when the animals prepare to fight. The dog stands erect, holds up his tail and bristles up the hairs on his back and shoulders; the cat crouches down with paws out and the tail laid flat on the ground, and gently waved from side to side. When the opposite emotions of gentleness, submission, and affection occur, the attitudes assumed are as remote as possible from those associated with anger and pugnacity.

The third principle is, that certain actions expressive of certain states of mind are the direct results of the constitution of the nervous system, being almost wholly independent of the will and of habit. Trembling under the influence of fear, or rage, or joy, is an example of this. It is of no use and it is quite involuntary; it cannot, therefore, have been acquired by the means already pointed out. It may be said that this is merely a confession of ignorance, and so it is in some cases; but in others Mr. Darwin traces the causes in the known action of certain nerves or muscles, and so gives a valid explanation. Such is the case with the firm closure of the eyes by screaming infants. This is quite involuntary, and does not occur later in life, but the whole mechanism by which it is produced has been traced out, and it is found that it is a provision to prevent injury to the delicate vessels of the eyes by the increased flow of blood to the head during violent screaming.

By means of a series of questions sent to correspondents in various parts of the world, Mr. Darwin has ascertained that many well-known modes of expression are almost universal. Even such an apparently conventional action as the shrug of helplessness or apologetic refusal has been observed among various savage races. Being thus proved to be a natural, not an acquired, expression, it becomes necessary to account for it, and this is done on the principle of antithesis; every part of the expression being the opposite of that which implies determination and action. Comparatively few human expressions, on the other hand, can be distinctly recognised in animals, that of sneering by raising the upper lip on one side, and thus showing the canine teeth, being one of the most curious. There is a very elaborate discussion on blushing. This is a peculiarly human attribute, being observed in almost every race of man, but not in the lower animals. It has been thought by some to be a special endowment for the purpose of expressing modesty or shame, but Mr. Darwin objects to this view, because it occurs in dark races, when it is hardly visible, and also because shyness is the most frequent cause of blushing, and this is of no use, and makes both the actor and spectator equally uncomfortable. The theory adopted is, that blushing is caused by self-consciousness directed chiefly to our personal appearance, and is therefore generally exhibited in the face, to which attention is most directed, and the skin of which is very sensitive. Much evidence is adduced to show that attention directed to any part or organ can affect its condition or action, and this is the physiological fact on which the explanation rests. Great confusion of mind often accompanies blushing, and is supposed to be caused by it. But it seems more probable that it is caused by the whole attention being so powerfully directed to ourselves as to interfere with the action of the mind in any other direction. A remarkable instance of this confusion is given by Mr. Darwin on the authority of an eyewitness:—

"A small dinner party was given in honour of an extremely shy man, who, when he rose to return thanks, rehearsed the speech, which he had evidently learnt by heart, in absolute silence, and did not utter a single word; but he acted as if he were speaking with much emphasis. His friends perceiving how the case stood, loudly applauded the imaginary bursts of eloquence whenever his gestures indicated a pause; and the man never discovered that he had remained the whole time completely silent. On the contrary, he afterwards remarked to my friend with much satisfaction that he thought he had succeeded uncommonly well."

It has been an objection to Mr. Darwin's theory of the "Origin of Species," that the rattlesnake warns its prey of its vicinity, and that such a habit could not possibly have been acquired by natural selection. In a very interesting discussion on the means of exciting fear in an enemy, Mr. Darwin gives a fuller statement of his views on this subject than he has done in any of his former works. He finds that various kinds of reptiles inflate themselves, hiss, open their mouths, and assume a ferocious aspect as a means of protection against attack. The cobra dilates its hood when alarmed or excited, and the puff adder swells and hisses with a sound hardly distinguishable from the rattle of the rattlesnake. He believes, therefore, that all these various sounds and appearances are warnings to would-be devourers that the creatures who produce them are dangerous. The rattle of the rattlesnake is said to imitate closely the sound of a cicada inhabiting the same region, and it has been supposed that it serves the purpose of attracting insect-eating birds as the snake's prey; but this view is rendered improbable by the fact that the snake rattles when alarmed or threatened. If it is proved to be a warning to enemies, it becomes useful to the creature itself, and could, therefore, have been acquired by natural selection.

In some cases the explanations given seem far-fetched, or simpler ones appear to be overlooked. I can hardly believe that when a cat, lying on a shawl or other soft material, pats or pounds it with its feet, or sometimes sucks a piece of it, it is the persistence of the habit of pressing the mammary glands and sucking during kittenhood; nor that the frequent practice of cats rubbing against their master's legs is derived from the habit of fondling their young. The habits and ideas of infancy seem to be completely lost in adult life, and to be replaced by others widely different; and it seems hardly likely that they should persist so strongly in one or two isolated instances without leaving more frequent and less equivocal traces behind them.

When a horse breaks into a gallop, at full speed, he always lowers his tail, and this is said to be done in order that as little resistance as possible may be offered to the air. This reason seems very fanciful, when the obvious explanation occurs, that, as the whole available nervous energy is being expended in locomotion, all special muscular contractions not aiding in the motion cease. It also seems very unsatisfactory to refer the vague and undefined yet deep emotions often excited by music to a recalling or survival of "strong emotions felt during long past ages, when, as is probable, our early progenitors courted each other by the aid of vocal tones," although it is very difficult to suggest any other explanation.

The open mouth, and raised arms with open hands turned outwards, is an expression of astonishment very general all over the world. Mr. Darwin explains the open mouth by a complication of causes, but he omits to notice, what seems to me a very probable one, that it represents an incipient cry of alarm or fear, or call for help. The raising of the arms and the open hands are explained by antithesis, they being the opposite of a state of indifference or listlessness. But this seems very unsatisfactory. The attitude is too definite, too uniform, and too widespread, to be derived from such a vague and variable cause as the opposite of a position of unconcernedness. There seems, however, to be a very obvious and natural explanation of the gesture. Astonishment, among our savage ancestors, would most frequently be excited by the sudden appearance of enemies or wild beasts, or by seeing a friend or a child in imminent danger. The appropriate movement, either to defend the observer's face or body, or to prepare to give assistance to the person in danger, is to raise the arms and open the hands, at the same time opening the mouth to utter a cry of alarm or encouragement. It is the protective attitude of an unarmed man to be ready to ward off attack of some uncertain or undefined kind; and very nearly the same attitude is that which we adopt as we rush to the assistance of some one in danger, our hands ready to grasp and save him. When used by us as a mere sign of astonishment, at some strange but harmless phenomenon, it has become to a great extent conventional, but the origin here advocated is rendered probable by a remark of Mr. Darwin himself, that, as one of the expressions of fear, "the arms may be protruded as if to avert some dreadful danger;" and among savages almost every source of astonishment would excite more or less fear.

It is rather curious that an author who is not usually satisfied with anything less than a real and intelligible explanation, should yet be so ready, in some cases, to admit innate ideas or feelings. Among the numerous, and often most interesting, observations on his own children, Mr. Darwin tells us that a child six months old was distressed at seeing its nurse pretend to cry. He thinks, in this case, that "an innate feeling must have told him that the pretended crying of his nurse expressed grief; and this, through the instinct of sympathy, excited grief in him." Now, although I imagined myself much more disposed to believe in innate ideas than Mr. Darwin, I cannot see the necessity for them here. A child at that age often cries or is distressed at any strange face, or even at the sight of a friend in a strange dress. The nurse's attitude and expression were strange; they made her look unlike herself, and the child got afraid, and was about to cry. That seems to me a better explanation than that the child had an innate knowledge that the nurse was grieved.

Somewhat akin to this is a readiness to accept the most marvellous conclusions or interpretations of physiologists on what seem very insufficient grounds. In discussing the subject of reflex action Mr. Darwin quotes the well-known experiment of the decapitated frog, which is said to wipe off a drop of acid from its thigh by a motion of the foot of the same leg. But if this foot is cut off it makes several fruitless efforts, then stops a while, as if restless and seeking some other way, and then, *by using the other foot, succeeds in wiping off the drop of acid.* Now this is imputed to pure reflex action, and not a word of doubt is thrown either on the experiment or on the inference from it. Yet it seems to me absolutely certain, either that the experiment is not correctly recorded, or that, if correct, it demonstrates volition and not reflex action. For surely reflex action cannot produce, in a decapitated frog, movements which were probably never once performed by the living frog. The action of drawing up the leg in swimming or leaping is one which the frog performs incessantly during its whole life; it would therefore probably be performed under any suitable stimulus by reflex action, and might, as a consequence of the usual motions, wipe off the drop of acid from a place which the foot, during contraction, would naturally reach. But the action of crossing one foot over to the thigh of the other leg is one which was very rarely, if ever, performed, because during life the frog possessed both its feet. Again, reflex action cannot be set up without a suitable stimulus. The stimulus applied to one leg set up reflex action in that leg, or perhaps, by co-ordination, of the muscular movements in the two legs; but, when one foot was cut off, what caused the nature of the motion to change, and a new set of muscles to be called into action, with such precision as to apply the foot to an unaccustomed part of the body? This is the work of consciousness; first to *know* that the one motion failed to produce an effect aimed at, next to change the motion so as to produce the *desired* effect. The experiment is described as if all this were really done by reflex action; but, if so, then what need have we of consciousness in animals at all, and why may not all their motions and actions during life be so produced? If the experiment, as recorded, is strictly accurate, it appears to me to demonstrate consciousness and volition, on the part of the frog, without a brain,—a fact by no means incredible in itself, but one which, if established, might have important consequences.

The book is admirably illustrated, both by woodcuts and by a number of photographs representing the most characteristic expressions. It is written with all the author's usual clearness and precision; and although some parts are a little tedious, from the amount of minute detail required, there is throughout so much of acute observation and amusing anecdote as to render it perhaps more attractive to general readers than any of Mr. Darwin's previous works.

ALFRED R. WALLACE.

The Hygiene of Air and Water: being a Popular Account of the Effects of the Impurities of Air and Water, their Detection, and the Modes of Remedying them. By WILLIAM PROCTER, M.D., F.C.S., Surgeon to the York Dispensary, and formerly Lecturer on Chemistry and Forensic Medicine in the York School of Medicine. London: R. Hardwicke. York: Sampson, Pickering, Johnson, and Tesseyman. 1872. 79 pp.

THE Science of Health in these days is making great advance, and asserts increasing claims for recognition. Its position is a difficult one, for whilst it of necessity lays under contribution the latest discoveries and most abstruse doctrines of modern thought, it must be translated for the comprehension of the bulk of people of the world who have themselves to carry out the precepts which it inculcates. Unfortunately the efforts of the interpreters between Science and the Public are not always successful, and frothy phrases often constitute a large part of so-called popular manuals,—there is a minute morsel of bread to a prodigious quantity of sack. It is a relief to turn to Dr. Procter's little book, which seems to give us exactly what we want; it is correct in data, terse, practical, and smooth in diction. The author takes a very modest position. He says:—"As treated in the following pages the subject admits of no originality, and the author claims none; his object has been to deal with it in as simple and popular a manner as possible, and to point out the injurious effects produced on health by impure air and water, the sources and origin of their impurities, with the means for their detection, and the several methods by which they may be removed or remedied." Taking the first two pages as a test of the amount of information conveyed, we find, in the course of a brief discussion of the causes of atmospheric impurity, a statement of the normal constituents of atmospheric air, their properties and quantitative relation, the preparation and uses of ozone and the method of testing for its presence, and a word or two concerning the suspended impurities of air. The work goes on to consider the causes which render air impure, the effects of respiration, putrefactive emanations, sewer gas (with useful hints for remedying it), the methods of detecting organic impurity in air, the natural laws for the purification of the atmosphere, ventilation, disinfection, and the hygiene of the sick room.

The second part of the book deals with the impurities of water and their removal. The causes and effects of water contamination, and the relation of typhoid fever and cholera to impure water (a subject on which people will find it greatly to their interest to be enlightened), are well, but briefly, discussed. The description of methods of detecting the impurities of water is succinct, but yet up to the time. Dr. Burdon Sanderson's "zymotic test" is noted, and the methods of detection of nitrogenous matter are simply put. The little volume concludes with hints on the removal of impurities from water.

We recommend it to all; to those whose scientific labours, directly or indirectly, tend to advance or to apply the knowledge of hygiene,—they will find it a useful compendium; to all others whose occupation is in other grooves, but who nevertheless have a personal interest in the preservation of health,—they will find it an easily intelligible and most valuable guide.

A Manual of Microscopic Mounting; with Notes on the Collection and Examination of Objects. By JOHN H. MARTIN, Author of "Microscopic Objects," &c. Illustrations drawn by the Author. 200 pp., 8vo. 11 plates. London: J. and A. Churchill.

THE subject-matter of this volume is divided into seven chapters and an appendix.

The first treats on various apparatus employed; in many instances directions are given for construction, and some of the author's own contrivances are described. Chapters 2, 3, 4, describe practically the methods of mounting objects dry, in balsam and solution of damar, and in fluids. The system of selecting a list of typical and easily procured objects is a good one: each object so selected is treated separately, and by following out the processes described in these chapters the student who is deprived of the help of more experienced workers will be able to make considerable progress. The author is evidently too fond of the old method of potass maceration in making preparations of insects. The flea as prepared by him is the mere empty skin so common in cabinets: this has only to be compared with specimens mounted in glycerine, without compression, with the contents of the body *in situ*, to cause it to be abandoned, excepting in those cases where the chitinous tissues alone are required. The proboscis of the blow-fly, again, is so treated as to produce the common preparation of the shops,—a mode of mounting which has for years only served to prevent a true knowledge of the structure of this wonderfully complex organ from being obtained. The author has surely never seen some of the insect preparations *au naturel*, which are now far from uncommon in the cabinets of some of our best microscopists.

A great deal of useful information is contained in chapter 5, giving a general summary of various modes of mounting. A large number of interesting objects are here described, and directions given for their examination.

The chapter on Collection gives a great many hints for capturing the small game so much sought after by the microscopist.

Some notice is taken of the important subject of adulterations, but the treatment is so brief—giving little more than a catalogue of adulterating substances—that the information will prove of but little use to the reader. At page 167 the author gives a figure of "a precipitating cell" of his own contrivance, but has unfortunately left out all description, so that the reader is left to make out what he can from the woodcut.

The appendix is one of the most useful portions of the book, containing no less than seventy-seven formulæ for various cements, mounting media, reagents, &c. This microscopic pharmacopœia, compiled from various sources, supplies a real want, and will be duly appreciated.

With regard to the illustrations, the author has certainly not improved in his lithography since the issue of his work on "Microscopic Objects." This is much to be regretted, as Plate 11—a reproduction of some of the author's drawings by the photo-lithographic process—shows that the defect is a want of skill in the manipulation of the lithographic materials. The other plates are characterised by a general coarseness of execution. The figure of flax, Pl. 10, Fig. 92, is unlike any fibre known to the histologist, and the whole plate is a specimen of very coarse wood-engraving. It is a pity that the book should have been spoiled by the bad execution of so important a portion.

With so many admirable existing manuals the present work was scarcely needed: it would have been better if the small amount of new matter had found its way into the pages of one of the periodicals devoted to microscopical subjects.

Records of the Rocks; or Notes on the Geology, Natural History, and Antiquities of North and South Wales, Devon, and Cornwall. By Rev. W. S. SYMONDS, F.G.S., Rector of Pendock. With numerous Illustrations. London: John Murray. 1872.

No one need be afraid that he will be led into any discussion of the attitude of either science in general or geology in particular in reference to the Bible or ordinary religious teaching. The title, so similar to Hugh Miller's "Testimony of the Rocks," and the clerical position of the author, might lead to this supposition. But not a word of the kind is to be found in the book; in fact the latter part of the title is really a fair exposition of its contents. Mr. Symonds evidently knows his country well, has walked it over and over again, has studied Sir Roderick Murchison's "Silurian System and Siluria" thoroughly, and has given the world the results of his observations. The geology naturally is the principal part of the work, and the order of the work follows that of the Rocks, beginning with the Laurentian, and ending with the Permian. A devout adherent of Sir Roderick Murchison, the author not only follows him over the same ground, but he adopts his theories entirely, and owes very many of his woodcuts to him. The remaining illustrations, mostly by Sir Wm. Guise, are well and carefully drawn. The natural history portion of the work consists mainly of a record of the habitats of rather rare plants, and the resort of various fish; whilst the antiquarian part of the work is the weakest of all, being merely the accounts such as might be found in ordinary guide books of old castles, with an occasional quotation from an ancient chronicler of a passage, the critical authority of which is not very minutely examined. Altogether the book will be found useful by those who are going over the country described, for whilst it is more portable, it also contains more minute detail than "Siluria," and touches upon subjects not alluded to in the other, in all respects, greater work.

A Budget of Paradoxes. By AUGUSTUS DE MORGAN, F.R.A.S., and C.P.S. of Trinity College, Cambridge (Reprinted with the Author's Additions from the "Athenæum"). London: Longmans. 1872.

MANY of our readers as they peruse the title of this book will recall with regret a quaint little figure, usually attired in a broadtailed dress coat, with an old-fashioned white tie of prodigious dimensions, round spectacles well stopped out with thick black rims, and a small mouth looking very grave, but with a pucker about the corners that betrayed a volcano of fun beneath ever ready to *erupt*. Such was the Budgeteer of Paradoxes. A shrewd thinker, as deep both as a logician and as a mathematician as any of his contemporaries (and he reckoned among his friends Airy, Babbage, Sir John Herschel, and Whewell), he had a fund of humour, and good humour, that one could scarcely have thought could have expended itself on exact science; hence, we may say, arose this collection of inexact science, falsely so called, brought together for the warning and encouragement of future enquirers, and for the amusement of lookers on.

The word "paradox" as used in this book is explained to mean "something which is apart from general opinion either in subject matter or conclusion;" consequently mixed up with the most good humoured banter at circle squarers, trisectors of angles, duplicators of the square, maintainers of the non-rotation of the moon, deniers of gravitation, the rotation and spherical shape of the earth, the discoverers of perpetual motion, the philosopher's stone, exact laws of meteorology, the exponents of the number of the beast, and other discoveries which the world does not as yet believe in; we find also discussions of the theories and accounts of some of the works of Roger Bacon, Francis Bacon, William Gilbert, Thomas Hobbes, Bishop Wilkins, Sir Isaac Newton, Sir Matthew Hale, Sir Kenelm Digby, Sir George Cornewall Lewis, the early researches of the Royal Society, and many other matters by which the aggregate of our knowledge has been increased. The object of the book is stated to be "to enable those who have been puzzled by one or two discoverers to see how they look in the lump; and incidentally to this we have drawn most clearly a distinction between those who have really made great discoveries and those who have wasted great ingenuity or labour upon what has proved useless; and this is done by showing that it is vain for a man to attempt to improve the knowledge of the world upon any particular subject until he knows all that has been done in that subject. Many of the circle squarers, for instance, are utterly unaware that it has been proved incontrovertibly that it is impossible to arrive at the *exact arithmetical* proportion between the diameter and circumference, but that nevertheless in this very direction the calculation has been carried out to 607 decimal places, a degree of accuracy far greater than is ever required for any practical purposes; so great, indeed, that few persons can realise the extent of its accuracy. It has never, indeed, been proved that it is impossible to produce a *geometrical* equivalent for the circle, but this does not attract so many theorisers. In the collection before us, which is confessedly imperfect, and only consists of the works actually in Professor De Morgan's possession up to 1867, we find man after man assigning a certain exact sum as this ratio, every man a different amount, and every man confident not only that he alone is right, but that were it not for pride and obstinacy or some such feelings the great mathematicians and astronomers must acknowledge him to be so. These men think they have made lucky hits; but the real discoverers, those whose opinions were at first deemed absurd, but have afterwards convinced the world, such as Galileo, Copernicus, Harvey, and Jenner, have patiently won their way through all previously attained knowledge, making sure of each step as they went along, and then building upon the foundation already laid; thus they have raised themselves above the level of their day. All this is drawn out with much humour and great kindliness of feeling, and so the book is one which is calculated to do great good to those who fancy that they have made great discoveries, whilst they have omitted to acquire the necessary qualifications for discovery, by showing how others have failed in similar pursuits, and also to those who have the power of enlarging our knowledge by encouraging them to proceed in spite of the opposition of the ignorant, after they have assured themselves of all the preliminary steps.

In a work of such varied contents, and so brimful of humour, it is impossible almost to make fair selections. The editor herself evidently has felt this, for whilst she acknowledges that there are repetitions and redundancies, she has found it impossible to cut out these flaws without materially damaging the work. Many of the peculiarities of the writer naturally exhibit themselves in a work of this kind. Many a good story about mathematicians, and especially Cambridge men; many anagrams, evidently a favourite amusement with the author; a few striking remarks about language; and not a few additions to the English language, will afford pleasure to many who would not care much for the mathematical part of the work. A liberal and highly independent view of politics and theology, which one cannot but admire in the man, rather disfigure a work professedly on other subjects. At the same time we miss some discussions which we were entitled to expect, notably the writings of Professor Piazzi Smyth on the Pyramids, who is dismissed with a single casual sentence in the middle of an article on another subject, though his predecessor in the same discussion, Mr. John Taylor, receives longer notice but no criticism of his results. On page 236, immediately before the discussion of the share that Adams and Le Verrier took in the discovery of Neptune, there is a rather glaring misprint: 1826 should be read 1846. On page 385 also there is a discussion of the word aneroid founded on a mistaken derivation; it was formed by the discoverer of the instrument from α, privative, and νηρὸς, moist, because no liquid was employed in this measure of the atmospheric pressure. Our old friend *bogy* is misspelt *boguey*. A few words new to the English language occur occasionally as an "*almamaternal* brother," "*antipharmacopœal* drenches," a "*sphragidonychangocometical* fellow," "geoplatylogical lectures."

The Orbs Around Us: A Series of Familiar Essays on the Moon and Planets, Meteors and Comets, Suns and Coloured Pairs of Suns. By RICHARD A. PROCTOR, B.A. (Camb.), Hon. Sec. R.A.S., Author of "The Sun," "Other Worlds than Ours," &c. London: Longmans and Co. 1872.

THE well-known author of the interesting essay on "The Sun" has become even still more popular by the publication of subsequent works upon the planetary system. But he has looked back upon his work, and found that the series of descriptions of "Other Worlds than Ours" might, in his estimation, be made to embrace a larger class of readers, if there were appended an introduction or explanation. Careful not to explain too much, Mr. Proctor has supplemented the work just now mentioned with the one before us, on "The Orbs Around us." We will state its salient points. The first essay is intended to elucidate the mysteries of the spectroscope for those who have but a very slight appreciation of the details of this mode of research. The succeeding essays, on the subject of the plurality of worlds, are especially interesting; but even these are exceeded by that entitled "The Rosse Telescope Set to New Work." The value of the work, however, centres in the first essay, because its comprehension includes the capability of progress into more intricate branches of the science of spectral analysis. Mr. Proctor's mission is pre-eminently that of a great teacher of scientific first principles; and his books should be read by all who desire to grasp, if not the detail, at least the liberal ideas of astronomical science. There is no science whose views are so extended, and we may be pardoned if we say that there are few so qualified to impart a knowledge of these views as Mr. Proctor.

The Strength of Materials and Structures. By JOHN ANDERSON, C.E., LL.D., F.R.S.E. London: Longmans and Co. 1872.

THIS treatise is one of the series of the Text-Books of Science now in course of publication by Messrs. Longmans. It is divided into two distinct parts. The first part treats of the natural properties of various materials employed in construction, as far as these qualities and characteristics are of importance to the engineer. In this way the leading peculiarities of cast-iron and wrought-iron, steel, copper, alloys, timber, &c., are described. The student, in the second division of the work, is instructed how to combine materials so as to obtain maximum strength at a minimum of cost and weight.

The work is fully equal to its predecessors, and is characterised by the usual care for accuracy where tables are concerned. The engineering student should at once add it to his library.

Notes on River Basins. By Robert A. Williams. London: Longmans and Co. 1872.

This is a collection of short notes on river basins, drawn up from the works of Petermann and Milner, Mackay, Long and Porter, McLeod, and others. The source, course, drainage, mouth, and tributaries of each river are given; and the area and other details of the lakes of England, Scotland, and Ireland are clearly laid down. The work appears well adapted to the use of pupil-teachers and schoolmasters.

Reports on Observations of Encke's Comet during its Return in 1871. By Asaph Hall and Wm. Harkness, Professors of Mathematics, U.S. Navy. Washington. 1872.

The astronomer and those interested in the science of Astronomy will welcome this able pamphlet. Many difficulties have occurred in the observation, especially in the use of the spectroscope. The spectrum of the comet was very faint; hence it was necessary to remove the photographed micrometer scale of the spectroscope. In its place was inserted a brass plate, pierced with a hole 0·00796 of an inch in diameter, moved by means of a micrometer-screw. The light passing through the hole is reflected from the surface of the prism, and appears, in the field of view of the spectroscope telescope, as a bright disc, with an apparent diameter of 36′ 55″, which can be made to traverse the whole length of spectrum by turning the micrometer-screw. "The illumination of the disc can be adjusted to the brightness of the spectrum under observation with the greatest nicety. If it is required to be very brilliant, the direct light of a lantern may be thrown into the hole: a less degree of brightness may be secured by passing the light through a piece of ground-glass; and finally, the luminosity may be varied down to absolute invisibility by reflecting the light into the hole from the back of the observer's hand held at a suitable angle. This last plan was employed in the case of the comet. The micrometer head is half an inch in diameter, and divided to one-tenth of a revolution, while each complete revolution of the screw moves the brass plate 0·0181 of an inch, which corresponds to an angular distance of 14′ 40·5″."

In using this micrometer, the readings on the line whose place was to be determined were habitually made alternately with readings on a sodium-line, produced by the flame of an alcohol-lamp with a salted wick held before the object-glass of the large telescope. The measures are thus entirely differential, and there is no risk of errors having been introduced by undetected changes of zero.

We may summarise the results of the observations in a few sentences:—Encke's comet gives a carbon spectrum. There is no polarisation to be detected in the light of the comet. The mass is certainly not less than that of an asteroid. The density of the supposed resisting medium in space, as computed from the retardation of the comet, is such that it would support a column of mercury between $\frac{220}{10^{17}}$ and $\frac{285}{10^{20}}$ of an inch in height. There is some probability that the electric currents which give rise to auroras are propagated in a medium which pervades all space, and that the spectrum of the aurora is, in reality, the spectrum of that medium. It is not improbable that the tails of all *large* comets will be found to give spectra similar to that of the aurora, although additional lines may be present.

In conclusion it may be said that, from the clearness of the detail, this pamphlet will be useful to the astronomical student.

The Forces of Nature. A Popular Introduction to the Study of Physical Phenomena. By AMEDEE GUILLEMIN. Translated from the French by Mrs. NORMAN LOCKYER; and Edited, with Additions and Notes, by J. NORMAN LOCKYER, F.R.S. London: Macmillan. 1872.

THE progress of Physical Science is nowhere more clearly apparent than in a comparison of the mode of producing its records. The soberly bound volumes of half a century ago are not more likely to be banished to the higher shelves of our bookcases because the theories they expound are obsolete, than they are to be superseded by the luxuriously printed and illustrated books in which the philosopher of to-day declares the laws of Nature according to his present lights. It is fit it should be so. Delicate instruments and logical reasoning should have their details drawn with a loving hand. Much of the science of yesterday lived grimly and darkly in its own study; the science of to-day throws its light upon all, and as a natural truth should be shown as it appears, in its own attractive form. For instance, why should not the diary of a journey through the realms of light—" a fairy-like, enchanted world, a world of wonders, where rubies, sapphires, topazes, and all kinds of precious stones send forth their fires, where every object is of incomparable beauty and splendour"—receive the most efficient ornament the aid of art can impart. Such a luxury, if it is luxury, is a practical one, for it raises in the mind of the student the enthusiasm which is necessary to render him a lover of not only Nature, but, as well, of Nature's laws.

The book which we have to notice is of French origin, from the pen of the celebrated Guillemin; its appearance in England is due to the united labours of Mrs. and Mr. Norman Lockyer. We need but say that it contains all the information of other works on Physical Science, under the heads of Gravity, Sound, Heat, Electricity, and Light, and that this information is further aided by the most eloquent and vivid illustrations we should think the power of the artist could attain."

A Treatise on the Building and Ornamental Stones of Great Britain and Foreign Countries. By EDWARD HULL, M.A., F.R.S., Director of the Geological Survey of Ireland, Professor of Geology in the Royal College of Science, Dublin. London: Macmillan and Co. 1872.

THOSE interested in the geological distribution and mineral character of the building and ornamental stones employed in the erection of ancient and modern structures will be pleased to find that the materials, which have hitherto been scattered so widely, have been brought within the limits of a single volume. Building and ornamental stones have not, we believe, been described in a complete manner, nor with any particular scientific arrangement. The engineer or student in Ireland is better provided for by Mr. G. Wilkinson's "Ancient Architecture and Practical Geology of Ireland; in France, M. T. Chateau has published his "Technologie du Bâtiment."

Under the general divisions of granitic, porphyritic, greenstone, and serpentinous rocks, marbles, alabasters, the rarer ornamental stones, calcareous and siliceous stones, tufaceous stone and slates, Mr. Hull deals with the varieties found in different parts of the world, in a manner clear, concise, but sufficiently detailed. One of the concluding chapters, on the selection of building stones with special regard to climate and the nature of the atmosphere, is well worthy the attention of the practical engineer. In each and all its departments the work is a valuable addition to our engineering literature.

Life of Richārd Trevithick, with an Account of his Inventions. By FRANCIS TREVITHICK, C.E. Vol. II. London: E. and F. N. Spon. 1872.

THE fertility of Trevithick's inventive powers appears to even greater advantage in this second volume than in the first, which we recently had occasion to notice. Although so many of Trevithick's ideas have been superseded by later inventions, there are several schemes which in the present day would afford valuable application. The engineering student should read the work as a record of many difficulties surmounted, and as many more avoided; it embodies both precept and example.

A Manual of Palæontology. By HENRY ALLEYNE NICHOLSON, M.D., D.Sc., &c., Professor of Natural History and Botany in University College, Toronto. Edinburgh and London: Blackwood and Sons. 1872.

DR. NICHOLSON'S object has been to furnish the student of geology and the general reader with a compendious account of the leading principles and facts of the vast and ever-increasing science of Palæontology. The work is divided under four heads: —the first includes a general account of the principles upon which the palæontological observer proceeds; the second treats of the past history of the animal kingdom, devoting much more space than is generally accorded to the consideration of invertebrate groups; under the third head is given a comprehensive view of palæobotany, or the past history of the vegetable kingdom; while, finally, the author applies the principles of palæontological science to the elucidation of the succession of the stratified deposits of the earth's crust. To say that this is the best handbook yet produced by the prolific pen of Dr. Nicholson is to accord the highest praise. The work is profusely and well illustrated.

Elements of Zoology. By ANDREW WILSON, Lecturer on Zoology, Edinburgh. Edinburgh: Adam and Black. 1873.

THIS is a manual intended to convey the principle of the division of zoological science to the student of an elementary course. The explanation is terse, but sufficient; the illustrations are numerous and well selected.

A Manual of Elementary Chemistry, Theoretical and Practical. By GEORGE FOWNES, F.R.S., late Professor of Practical Chemistry in University College, London. Eleventh Edition. Revised and Corrected by HENRY WATTS, B.A., F.R.S. London: J. and A. Churchill. 1872.

THE eleventh edition of this well-known manual of chemistry presents some marked alterations. The work, under the careful editorship of Mr. Watts, fully keeps pace with the progress of chemical science. But the volume appears overgrown: if the matter were divided under the heads of organic and inorganic chemistry, and each portion included in a separate volume, the manual would take a much handier form. The present volume is too bulky to be held with ease. The use of a bolder type is a very considerable improvement. It is unnecessary to recommend the work more particularly.

Elementary Geology. A Course of Nine Lectures. By J. CLIFTON WARD, F.G.S., Associate of the Royal School of Mines; of Her Majesty's Geological Survey. London: Trübner and Co. 1872.

MR. WARD is already well known to the scholastic world by his work on Elementary Natural Philosophy. The present work is founded upon a similar plan, and is specially adapted for its proposed use by junior students and in schools.

Notes for My Students. Magnetism. By WILLIAM J. WILSON, F.C.S. London: J. Bale and Sons. 1872.

THIS little work is admirably adapted for the use of either the advanced or elementary student. It is very clearly and concisely written, and comprises much useful information.

The Causation of Sleep. By JAMES CAPPIE, M.D. Edinburgh: Thin. 1872.

DR. CAPPIE, in this essay, gives many novel and ingenious suggestions upon an interesting subject. It would be tedious to detail the many original views differing in some degree from the accepted opinions on a physiological subject. We recommend our readers to examine for themselves these arguments, which are clearly and logically stated in a sufficiently agreeable form.

PROGRESS IN SCIENCE.

MINING.

To-day—January, 1st, 1873—the two Mines Regulations Acts of last session come for the first time into operation. In conformity with certain sections of these Acts, every owner, agent, or manager of a mine must, before a specified date, forward to the inspector of his district a return of the annual produce of his mine. A complete change is, therefore, about to be introduced in the system of collecting our mineral statistics. Hitherto these valuable returns have been purely voluntary contributions, obtained through the personal influence of Mr. Robert Hunt, F.R.S., Keeper of Mining Records. As far back as 1847, statistics of this kind were for the first time collected and published by Mr. Hunt, and since 1853 the volumes have been regularly issued year by year—gradually growing in fullness and accuracy until they have assumed their present comprehensive form. In view of the compulsory system introduced by the new Acts, we may regard the volume for 1871[*]—which has appeared during the past quarter—as representing the last of the returns contributed by the courtesy of our British mine owners. From this volume we extract the following summary, showing the number of mines working in 1871, and the amount and value of the ores which they produced:—

Number of Mines.	Mineral.	Tons.	Cwts.	£
2760	Coal	117,352,028	—	35,205,608
210	Iron ore[†]	16,334,888	14	7,670,572
122	Copper ore	97,129	—	387,118
145	Tin ore	16,272	—	1,030,834
241	Lead ore	93,965	17	1,155,770
47	Zinc ore	17,736	10	56,330
33	Iron pyrites	61,973	—	64,987
1	Silver ore[‡]	5	—	421
16	Arsenic	4,147	15	15,519
9	Gossans, ochres, &c.	697	5	1,396
1	Wolfram and tungstate of soda[§]	20	—	228
1	Nickel[‖]	2	—	98
1	Bismuth[¶]	—	2	14
2	Fluor-spar	51	10	26
4	Manganese	5,548	1	22,958
1	Cobalt-ore[§]	3	—	120
	Barytes	5,512	8	3,539
	Clays, fine and fire	1,255,000	—	475,000
	Earthy minerals	—	—	600,000
	Salt	1,505,725	—	752,862
	Coprolites	36,500	—	51

Total value of the minerals produced in the United Kingdom in 1871 .. £47,494,400

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[*] Mineral Statistics of the United Kingdom of Great Britain and Ireland for the year 1871. With an Appendix. By Robert Hunt, F.R.S. London: Longmans and Stanford. 1872.

[†] "It has not been possible in every case to determine whether the return has been for *calcined* or *uncalcined* ore. The actual production of *raw* ore will probably be in excess of this quantity. Estimating the quantity of pig-iron at 2¾ tons of ore for each ton of iron, and deducting foreign ore, 'burnt ore,' and 'cinder' used, the quantity will be about or slightly above 17,000,000 tons."

[‡] From the Queen Mine, Calstock, Cornwall.

[§] From East Pool Mine, near Redruth, Cornwall.

[‖] From Silver Mine, Bathgate, Linlithgowshire.

[¶] From Dolcoath Mine, near Redruth, Cornwall.

From the official reports of the Inspectors of Coal Mines for 1871, we learn that 826 fatal accidents occurred in connection with our collieries during the year. It is true that this number is slightly less than the corresponding figures for the previous year; but whilst the 830 accidents of 1870 resulted in the loss of 991 lives, the 826 accidents of 1871 represent unhappily not fewer than 1075 lives. It appears that of every 345 colliers employed in 1871, one man perished by these accidents; or, to put the figures in another light, it may be said that one miner's life was sacrificed for every 109,246 tons of coal raised in that year. On analysing the list of fatal accidents, we find that 52 of them occurred through explosions of fire-damp, and resulted in 269 deaths; whilst 426 may be referred to falls of the coal, ironstone, or roof—a class of accidents which caused the loss of 435 lives. The remaining deaths were due to casualties in the shafts, and to miscellaneous accidents, both underground and at the surface. Let us hope that the working of the new Act may diminish each year this grim catalogue of colliery accidents.

Many of the Government inspectors introduce into their reports highly valuable suggestions, which merit the studious attention of all who are practically interested in our mining industries, and especially those who have the lives of our coal-miners in their charge. It is pleasing to mark the spirit in which some of the inspectors refer to the benefits which must accrue to mining officers from a scientific education, and to the influence which such training must needs exert on the intelligent discharge of their responsible duties. The means of acquiring such training are, however, not yet sufficiently extensive. Thus, Mr. Lionel Brough, after alluding with satisfaction to the establishment of the College of Physical Science at Newcastle-on-Tyne, maintains that "every centre should by right possess one of those most valuable educational establishments. The underground operations of Great Britain exceed those of any other nation in the world; therefore educational means should be provided proportionate to its immense mining industry."

As colliery explosions are often the indirect result of diminished atmospheric pressure, Mr. J. A. R. Newlands has suggested, with the view of preventing such calamities, that the air in coal mines should be maintained at a constant pressure by artificial means. To this end he proposes to cover the mouths of both the upcast and downcast shafts by air-tight chambers, sufficiently large to allow all the surface-work at the pit's mouth to be carried on within their walls. These chambers should be put in connection with powerful air-pumps, worked by steam-power, and a current of fresh air thus forced through the workings. This current could be so regulated that any desired degree of ventilation might be attained, while the air, if necessary, might be cooled, before passing into the pits, by compression in cylinders surrounded with cold water. When fire-damp makes it appearance, air should be drawn out of the mine, and the pressure in the workings thus diminished, so as to release, in the absence of the miners, any imprisoned gas. It is believed that in many collieries, dangerous accumulations of fire-damp might be prevented by the simpler plan of partially exhausting the air periodically, and then forcing a current of fresh air into the pit, so as to sweep through the entire system of workings. Instead, therefore, of erecting air-tight chambers, it would in such cases be merely necessary to cover the mouth of each shaft with an iron plate, having an aperture by which it could be put into communication with the pump for either exhausting or forcing-in the air.

We had occasion last quarter to mention that the Committee appointed by the War Office to report upon lithofracteur had come to the conclusion that this explosive is not perfectly safe under certain conditions. It is only fair, therefore, that we should now call attention to the fact that a different view has been taken by the Belgian Government, and that a concession has recently been granted for the transport and storage of lithofracteur in Belgium. A series of important experiments has been satisfactorily performed, on a large scale, with this substance before some of the chief mining and engineering authorities in that State. These experiments were made in some quarries of greenstone at Quenast, about 18 miles from Brussels, where a hard compact rock is largely quarried for paving and road-making. Without entering into the details of these experiments, which were conducted by Prof. Engels, the inventor of lithofracteur, we may say that they satisfactorily showed the extraordinary power and value of this explosive, whether for mining or for military blasting, and also demonstrated its incapacity to explode by fire or by ordinary percussion.

It seems highly probable that Eastern Australia will soon enter into competition with Cornwall and "The Straits" as a great tin-producing country. A report on the recent discoveries of tin-ore in the colony of Queensland was presented by Mr. F. T. Gregory at the opening meeting of the Geological Society this session. According to this document, the ore has already been found distributed over an area of about 550 square miles of granite country in the neighbourhood of the head-waters of the Severn River and its tributaries. Many small tin-lodes have been traced, invariably in association with a red granite; but the richest sources of tin are the deposits in the beds of streams and in the alluvial flats on their banks.

In the adjacent colony of New South Wales, and immediately adjoining the stanniferous region of Queensland, important discoveries of tin-ore have also been recently made, and are in course of rapid development. Some interesting observations on these discoveries have been transmitted to this country by Mr. G. H. F. Ulrich. The tin-yielding region of New South Wales forms an elevated plateau in the district of New England, and consists mainly of granitic and basaltic rocks, associated with metamorphic slates and sandstones. At the workings of the Elsmore Company, north-west of the Macintyre River, the granite is traversed by veins of quartz containing tin-stone, and by dykes of a softer granite, so rich in ore as to yield masses of oxide of tin up to at least 50 lbs. in weight. Capping the granite range is a layer of recent tin-bearing detritus, from 6 to 24 inches in thickness, and yielding from 3 ozs. to more than 2 lbs. of tin-ore per dish of about 20 lbs. Beneath this there occurs an older drift, which in some parts has yielded as much as 6 lbs. of ore per dish, whilst other parts are comparatively poor. Though the full development of the new mining industry thus established in this part of Australia may be to some extent restricted by lack of a sufficient supply of water, yet Mr. Ulrich considers it not unlikely that the production of tin-ore from this region will eventually reach, or even surpass, that of all the old tin-mining countries of the world. Mr. Daintree, who is well acquainted with the colony of Queensland, calculates that the value of the deposits of stream-tin in that colony must be about £13,000,000 sterling! And, assuming that the neighbouring colony of New South Wales possesses deposits of equal value, he estimates that the stream-tin of this eastern part of Australia amounts to about twenty-five times the annual production of Cornwall. The discovery of tin in New South Wales is said to be due to the Rev. W. B. Clarke, who in 1849 predicted the occurrence of this metal from the character of some of the local granites, and in 1853 reported the actual discovery of tin-ore in the neighbourhood of the Severn River. It is only lately, however, that these discoveries have excited any attention.

The first half-yearly part of a new official periodical—the "Annals of Mining in the Dutch East Indies"*—has lately been published. It contains some valuable geological, mining, and metallurgical articles, including an excellent paper on an important tin district in the island of Banca.

METALLURGY.

Perhaps the best idea of the importance of metallurgy among the industries of this country may be obtained by consulting the annual volumes of statistics issued from the Mining Record Office by Mr. R. Hunt, F.R.S. The returns for 1871 have been published during the past quarter, and from thence we learn that the value of metals produced from ores raised in this country during that year amounted to upwards of £20,000,000 sterling. The following summary exhibits the quantities and values of the several metals smelted from British ores in 1871:—

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* Jaarboek van het Mijnwezen in Nederlausch, Oost-Indië. Uitgegeven op last van zijne Excellentie die Minister van Koloniën. Eerste Jaargang; eerste Deel. 1872.

Pig-iron	Tons	6,627,179	£16,667,947
Copper	,,	6,280	475,143
Tin	,,	10,900	1,498,750
Lead	,,	69,056	1,251,815
Silver	Ozs.	761,490	190,372
Zinc	Tons	4,966	92,743
Other metals (estimated)			3,000
			£20,179,770

Certain improvements in the metallurgy of manganese have recently been effected by Mr. Hugo Tamm, and fully described in the "Chemical News." The ore employed in these investigations was an impure binoxide of manganese, containing 79·5 per cent of peroxide of manganese, 6·5 of peroxide of iron, 3·5 of water, and 10·5 of gangue, with traces of phosphate of lime. To obtain metallic manganese, 1000 parts of this ore are mixed with 91 of lamp-black or soot, and with 635 parts of a mixture described as "green flux." This is prepared in the following way:—A mixture is made of 63 parts of ground glass, 18½ of quick-lime, and 18½ of fluor-spar. Of this mixture 34 parts are taken and incorporated with 5½ of lamp-black and 60½ of native peroxide of manganese. On smelting this mixture, metallic manganese is obtained, accompanied by an olive-green slag: this slag, when ground, forms the green flux previously described. The charge of ore, flux, and carbonaceous matter, in the proportions indicated above, and moistened with oil, is introduced into a refractory crucible lined by a mixture made of 3 parts of plumbago and one of loam or fire-clay worked into a thick paste with water. The crucible is heated in a wind or blast-furnace, and a button of manganese obtained, together with the slag previously described. The metal reduced by this method is not pure manganese, but a product which the author designates as "cast manganese." A specimen of this contained—Metallic manganese, 96·9; iron, 1·05; aluminium, 0·1; calcium, 0·05; phosphorus, 0·05; sulphur, 0·05; silicon, 0·85; carbon, 0·95. The cast manganese may be refined by Berthier's process, which consists in re-melting the coarsely-powdered crude metal with carbonate of manganese. A sample of the refined metal obtained by this treatment had the following composition:—Manganese, 99·91; iron, 0·05; silicon, 0·015; carbon, 0·025. Mr. Tamm suggests that the cast manganese might be economically employed in certain operations as a good substitute for the alkaline metals.

A patent has been granted to Messrs. T. W. Gerhard and J. Light, jun., for the production of iron and steel from a certain preparation which they call "iron-coke." This is a mixture of powdered ore, or iron-scales, with a bituminous substance, such as pitch, and with carbonate of lime. Cast-iron may be obtained by smelting the iron-coke with ground coal or other carbonaceous matter. Wrought-iron may be procured by the reducing action of carbonic oxide generated in a combustion chamber connected with the furnace.

An improved method of lining rotatory puddling furnaces has been patented by Mr. Danks. Lime and oxide of iron, or silicate of iron, mixed in certain cases with soda, potash, or even common salt, are worked-up into the consistence of a stiff mortar, with which the revolving cylinder is lined. When this coating has become dry, iron ore is introduced into the furnace and melted, thus forming a vitreous lining. More ore, or oxide of iron, is then melted, and lumps of ore thrown into the molten mass, so that, when the liquid sets, the ends of these lumps project from the surface. In this condition the furnace is ready for puddling.

Certain salts—such as the alkaline nitrates and chlorates—are applied by Mr. R. Elsdon to the conversion of cast-iron into wrought-iron or steel by causing them to act on the *upper* surface of the molten pig-iron, which is placed in a peculiar syphon-shaped converter.

Mr. W. Dingley has lately patented the use of sulphate of soda, in the crude state of *salt-cake*, for the purification of iron. A small quantity of the salt is thrown on to the surface of the molten iron during the operation of puddling —a dose of about 12 ozs. being recommended for each heat of 4 or 4½ cwts. of metal.

Some improvements in the separation of silver and gold from lead have been announced by Messrs. Risway and Pauville, of Paris. The argentiferous or auriferous lead is treated with magnesium or aluminium, either alone or alloyed with zinc, and the rich scum thus obtained is amalgamated with mercury. The inventors state that they are able to regenerate the metals which have been used in the process of extraction, and have thus greatly reduced the expense of separating the precious metals from the lead.

Mr. F. Claudet has presented to the Academy of Sciences of Paris a memoir on his process of extracting gold and silver from burnt coppery pyrites—a process extensively conducted at Widnes by Mr. J. A. Phillips. The treatment consists essentially in roasting the burnt ore at a low temperature with common salt, lixiviating the product with water acidulated with hydrochloric acid, precipitating the silver by iodide of potassium, and decomposing the iodide of silver by metallic zinc. It is unnecessary, however, to enter into details of the process, as it was described by Mr. Phillips at the Liverpool meeting of the British Association. We learn from Mr. Claudet that in 1871 not less than 16,300 tons of burnt pyrites were thus treated at Widnes, and yielded 333·242 kilogrammes of silver and 3·172 kilogrammes of gold.

A capital account of tin-smelting, as practised in Banca, appeared in the first part of the new Dutch periodical on East Indian Mining. The description is written by Van Diest, and is illustrated by an effective chromolithograph representing the Chinese method of tin-smelting as practised at night.

MINERALOGY.

Last quarter we had occasion to refer briefly to the discovery of a new lead-bearing mineral called *Maxite*. A full description of this interesting species has since been published by Dr. Laspeyres.* Herr Max Braun, of the Vieille Montagne Zinc Mines, near Aix-la-Chapelle, having visited the lead mine known as the Mala Calzetta, near the town of Iglesias, in Sardinia, brought home with him certain specimens of the new mineral, which were at first taken for mendipite or chloro-carbonate of lead. It was soon found, however, that no chlorine was present, and a full analysis has since revealed the following composition:—Water, 1·866; carbonic acid, 8·082; sulphuric acid, 8·140; protoxide of lead, 81·912. From these figures the following formula may be deduced (using the old atomic weights):—

$$5(PbO.SO_3)+9(PbO.CO_2)+PbO.5HO.$$

This formula corresponds to 31 per cent of sulphate of lead, 49 of carbonate of lead, and 20 of hydrated oxide of lead. Up to the present time we believe that the mineral has not been found *in situ*, but the few specimens yet known have all been obtained from the dressing-floor at the mine. During the process of dressing, the crystals have of course been subjected to attrition, and hence the surfaces are much rubbed and rounded, so that no crystalline faces have yet been found sufficiently distinct to admit of measurement. It is inferred, however, from its cleavage and from its optical properties, that maxite belongs to the rhombic system. It presents the form of a colourless or greyish-yellow crystalline substance, with a pearly adamantine lustre on the cleavage planes. Its hardness is almost equal to that of calc-spar, and its specific gravity is 6·874. Optical examination shows that it is a negative doubly-refracting mineral. In many of its properties it is closely allied to the rare British mineral, leadhillite, from which it differs, however, in density, in the presence of water, and in certain other characteristics.

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* LEONHARDT und GEINITZ's Neues Jahrbuch für Mineralogie, U.S.W., 1872, Heft 5, p. 508; Journal für praktische Chemie, 1872, Heft 10, p. 470.

A new ore of mercury from Guadalcazar, in Mexico, has been described by Dr. T. Petersen, under the name of *Guadalcazarite*. It is a compact or crypto-crystalline iron-black mineral, with a metallic lustre and a black streak. The specific gravity is 7·15. An analysis yielded—Sulphur, 14·58; selenium, 1·08; mercury, 79·73; zinc, 4·23; with traces of cadmium and iron. Guadalcazarite is, therefore, a double sulphide of mercury and zinc, corresponding to the formula $6HgS+ZnS$, but with part of the sulphur replaced by selenium, and perhaps a small proportion of the zinc by cadmium.

In a recent number of the "Annales des Mines," M. Bertrand describes a curious yellow or reddish substance from the province of Los Bordos, in Chile, and found to contain the chlorides of silver and mercury, with oxide of mercury. It is believed that this substance is a mixture of two mineral species. One of these is a double chloride of silver and mercury, containing $AgCl$ 40·69, and Hg_2Cl 59·31 per cent: this new species is called *Bordosite*. The other constituent is ordinary protoxide of mercury, which in this native form is to be termed *Hydrargyrite*.

Under the name of *Syngenite*, Zepharovich has lately described a new mineral from Kalusz in Galicia. It occurs in the form of colourless tabular crystals, much resembling those of gypsum, and associated with crystals of sylvine, or chloride of potassium, in the salt mines of Kalusz. Syngenite is a hydrous double sulphate of calcium and potassium, somewhat resembling polyhalite, from which it differs in that it contains scarcely any sulphate of magnesium. The native crystals are almost identical with those of the similar product formed in the laboratory; but though belonging to the rhombic system, they affect a curiously deceptive monoclinic habit.

Herr H. Grüneberg has communicated to the German Chemical Society a memoir on the properties and economic applications of the mineral *Kieserite*. This is a hydrous sulphate of magnesium, containing only a single molecule of water, and hence differing from ordinary crystallised Epsom salts. Kieserite occurs abundantly among the "abraum salts," or deposits of magnesium and potassium salts in the upper beds of the salt mines at Stassfurt, near Magdeburg. It is extensively used in Manchester for dressing cotton and other fabrics, and it is also valued as a manure. Kieserite and common salt, reacting at a low temperature, furnish sulphate of sodium—hence another application of the mineral. Grüneberg has succeeded in preparing a double sulphate of magnesium and calcium by igniting a mixture of kieserite and gypsum, and has introduced this double salt as a hard and durable artificial stone.

At Nohl, near Kongelf in Sweden, Professor Nordenskjöld has discovered a new mineral which he terms *Nohlite*. This is a hydrated niobate of yttrium, uranium, zirconium, calcium, iron, &c. The mineral resembles the Uralian Samarskite, but contains more than 4·5 per cent of water.

Some few years ago, Mr. Ulrich, in his excellent notes on the mineralogy of Victoria, described some beautiful crystals of *Herschelite* from Chambers's basalt quarries at Richmond, near Melbourne. Samples of these crystals have lately been analysed by Herr Kerl in the laboratory of the University of Göttingen with the following results:—Silica, 43·7; alumina, 21·8; lime, 8·5; soda, 3·5; potash, traces; water, 22·2. Compared with the typical herschelite of Sicily, the Australian mineral contains much less silica, a larger proportion of water, and a notable difference in the proportion of lime and alkalies. Indeed a specimen of Sicilian herschelite contained only 0·31 per cent of lime, but as much as 8·84 of soda and 4·28 of potash. These differences in chemical composition have been considered sufficient to justify the separation of the Australian mineral from the species herschelite, in spite of the close agreement in the crystallographic characters of the two substances. Herr

M. Bauer, of Göttingen, has, therefore, proposed to distinguish the Australian mineral as *Seebachite*—a name complimentary to Professor Karl von Seebach. At the same time, it must be confessed that the composition of the two minerals may perhaps be eventually reduced to a general formula, in which event a relation might be traced between herschelite and seebachite similar to that which obtains between natrolite and mesolite.

A curious instance of the occurrence of hemimorphism in a crystal of calc-spar—perhaps an unique example—has also been described by Bauer in the *Zeitschrift* of the German Geological Society. The specimen in question occurs in a group of crystals of calcite from Andreasberg in the Hartz—a locality well known to the mineralogists for the beauty of its crystallised calcite. Most of the crystals in this group are attached by one end to the matrix, and hence it is impossible to compare the characters of the two extremities; but it happens that one crystal has curiously grown across another in such wise that the two ends of the former crystal are free, and hence admit of observation. The hemimorphism consists in one end being terminated simply by the flat basal plane, whilst the other extremity exhibits a complicated set of rhombohedra and scalenohedra. As the occurrence of hemimorphism is usually correlated with pyro-electric properties, the crystal of calcite was heated to 150° C., but without any development of electricity. Exposure to a higher temperature was forbidden by fear of damaging so interesting a specimen.

Mr. C. Horner has communicated to the "Chemical News" a short note, announcing the discovery of the rare metal didymium in a specimen of pyromorphite, or phosphate of lead, from Cumberland.

Dr. Gladstone, F.R.S., has succeeded in preparing microscopic specimens of filiform silver, strongly resembling the characteristic threads of the native metal well known to mineralogists as occurring in calcite at Kongsberg in Norway and in Chili. The artificial specimens were reduced from a solution of nitrate of silver by suboxide of copper, and it is suggested that the native silver may have been reduced by a similar reaction in nature.

ENGINEERING—CIVIL AND MECHANICAL.

Guns and Armour.—The results of the *Glatton-Hotspur* experiments recorded in our chronicles of last quarter have recently come under discussion at Portsmouth by the Naval Professional Association, on the 1st November last, when a paper on the subject was read by Commander W. Dawson, R.N. From this paper it appears that the defeat of the gun by the turret was in strict conformity with well-known mechanical principles, with previous Shoeburyness experiments, and with Woolwich calculations. The shots fired at the *Glatton* were from a 12-inch 25-ton gun, rifled on the French or Woolwich system, and the results of the firing fully confirmed Captain Hood's remark as to the well-known "inaccuracy of flight now observed in a 12-inch gun of 25 tons at very short ranges." Now, from a diagram published by Mr. N. Barnaby, the Chief Naval Architect, it appears that the force required to perforate the front of the *Glatton's* turret, at right angles, with a 12-inch projectile, is 7378 foot-tons. This would be exerted at 200 yards (the distance in question) by a 600 lb. shot, which left the gun at the rate of 1357 feet per second; or by a 700 lb. shot projected with an initial velocity of 1252 feet. But the 600 lb. shot actually employed, having those short stud rifle-bearings which, it is officially stated, have "decidedly the lowest velocities," left the gun with only 1300 feet velocity; and, travelling 23 feet per second slower at 200 yards distance, it struck the turret a blow of only 6788 foot-tons. In order that this projectile should perforate the front of the *Glatton's* turret, it must leave the gun with 1357 feet velocity, or with 57 feet greater initial velocity. The same object might be attained by propelling a 700 lb. projectile from the same gun with 105 feet less velocity than that requisite with a 600 lb. shot. Without following Commander Dawson through his proofs and arguments, in which he clearly traces the defeat of the gun to the defective system of rifling employed, and the short stud bearings of the shot necessitated by that system, it is sufficient to state that his conclusions are fully borne out by other authorities. Mr. Charles Merriman, F.R.S., the principal of the Royal School of Naval Architecture at South Kensington, says that "the consent of all mechanicians and engineers with "whom he has ever conversed was absolutely unanimous in the condemnation of the Woolwich system of rifling, and that he had never heard of any serious defence of it." The results of the *Glatton* experiment also confirm the unanimous testimony of unbiassed artillerists, of mechanicians, and of mathematicians, that the rifle system which "has decidedly the lowest velocities" has necessarily the least penetrating power; and Admiral A. C. Key, C.B., F.R.S., the former Director-General of Naval Ordnance, lays it down as a rule that without "penetrating power, at ranges up to 1200 yards, all other qualities are useless."

Railways.--Among the new lines of railway now approaching completion, we may notice the Mexican Railway, connecting the port of Vera Cruz with the capital of the country. It is 263 miles in length, with a branch 29 miles long to the city of Puebla. Leaving the port of Vera Cruz, the line runs up towards the mountains of the Chiquihuite, rising about 1600 feet in a distance of 53 miles; it then reaches the Tierra Templada, at a height of 4000 feet in 84 miles, and finally overcomes an elevation of 8043 feet on the borders of the great Mexican plateau, or Tierra Fria. In reaching this last elevation some very heavy work presents itself; steep gradients of 1 in 25 combined with curves of 350 feet radius, are frequent over a distance of 22 miles, and there are several viaducts and bridges of considerable size. The gauge of the line is 4 feet 8½ inches, and the engines, carriages, &c., will be all adapted to the sharp curves they will have to traverse.

The official list of new projects to be submitted to Parliament during the ensuing session comprises 280 plans of all classes, of which number 159 are railway schemes, 13 are tramway bills, and 65 are bills of the miscellaneous class. The railway schemes are chiefly provincial, although there are some which affect the metropolis. With regard to the latter there are, first, the City and West End Railway, which is a proposed line from the Metropolitan Railway at the Kensington Joint Station to Farringdon Street, the route being by way of Great Windmill Street to the Metropolitan Railway at the Farringdon Road Station. There is a new street between Tichborne Street and Rupert Street, and another between Holborn and Great Queen Street in connection with this scheme, besides which it is proposed to widen several streets along the route. The East and West Metropolitan Junction and Cannon Street Railway is a scheme for a line from the Metropolitan District Railway at Cannon Street to the Metropolitan Railway at Aldgate, to the East London Railway, and to the North London Railway at Bow. The Metropolitan and St. John's Wood Railway Company are seeking to connect their line with the Hampstead Junction Railway and the Midland Railway, and to construct a branch line to Kingsbury. The Hammersmith Extension Railway is a proposed line from the Metropolitan District Railway at Kensington to the Broadway, Hammersmith. The London Central Railway Company are seeking powers to form junction lines with the Great Northern Railway near the passenger station at King's Cross, and to effect a junction with the Metropolitan Railway near Osnaburg Street, Euston Road, and another junction with the same railway near Upper Fitzroy Street.

The Brighton, Eastbourne, and London Railway, which has already been before Parliament, is again brought forward. The Great Eastern and Felixstow is a line from the Westerfield station of the Great Eastern Railway to Felixstow. The Great Northern Railway propose to construct branches from their own line at Fletton to the London and North Western at Orton, and between their Nottingham and Grantham Branch at Barrowby to their main line at Barkston; they also seek powers for a railway from the termination of their authorised line at Melton Mowbray to Leicester, with three branch lines.

The Great Western Railway Company are seeking powers to make a line from Stourbridge to Kidderminster and Bewdley, some lines at Wrexham, sidings at Paddington and Bristol, and the extension of the Llwynennion branch. By another bill they propose to construct branches from the Cornwall Railway to Devonport, and from the West Cornwall Railway to St. Ives. The London and North Western Company are applying for powers to add considerably to their system by the construction of lines in Middlesex, Northampton, Rutland, Huntingdon, Stafford, Chester, York, Monmouth, Carmarthen, Glamorgan, and Carnarvon, which are too numerous to mention in detail. The South Western Railway propose a considerable extension of their system by the construction of lines in Bucks, Surrey, Berks, and Southampton. The Midland Railway propose several short branches at Stockingford, Kingsbury, Ripley, Teversall, Duckmanton, Skegley, Bestwood-Park, Holbeck, and one in connexion with the Metropolitan Railway at Whitecross Street. The South Eastern Railway Company are applying for powers to construct new lines at Rochester and Chatham, a junction line between the South Eastern, New Tunbridge, and Paddock Wood and Maidstone lines. The proposed Staines and West Drayton Railway is a line leaving the great Western line at Hillingdon, and terminating by a junction with the Windsor branch of the South Western Railway at Staines. The Swindon, Marlborough, and Andover Railway is a line from the Great Western, at Swindon, to the London and South Western at Andover.

Bridges.—An important engineering work is now under construction across the Thames, at Chelsea, known as the Albert Bridge. The principle of its construction is that known as Mr. Ordish's rigid suspension principle; this system consists in suspending the main girders, which carry the roadway, by straight inclined chains, which are maintained in their proper position by being suspended by vertical rods, at intervals of 20 feet, from a steel wire cable. The bridge, when completed, will have a total length of 710 feet, and a width of 41 feet between the parapets. These will be formed of the main girders, which are of wrought-iron, 8 feet deep and continuous, the upper portions being ornamentally perforated in order to lighten and improve the appearance of the structure. The main girders will be connected transversely by cross girders placed 8 feet apart, and on these will be laid the planking for the carriage roadway. There will be four towers carrying the main chains of the bridge, and they will be placed in pairs, each pair being connected at a height of 60 feet from the platform level by an ornamental iron arch. The towers are of cast-iron, and consist each of an inner column 4 feet in external diameter, surrounded by eight 12-inch octagonal columns placed 12 inches from the central shaft, the whole group being connected together at intervals by disc pieces or collars of cast-iron. The bridge is divided into a centre and two side openings, the former a span of 400 feet, and the latter 155 feet each. The foundations of the piers consist of cast-iron cylinders, the bottom or cutting ring being 21 feet in diameter, and they are the largest cylindrical castings ever made in one piece. From these the cylinders gradually taper to 15 feet in diameter at the level at which the towers commence. The cylinders are sunk down into the London clay, and then filled in with concrete.

A paper has recently been read before the American Society of Civil Engineers relative to the problem of how to sustain and maintain in position the arch ribs of the Illinois and St. Louis bridge during the progress of its construction. The most prominent novelty in the plan adopted consists in the absence of all scaffolding or trestling standing in the river, excepting only for a very short distance immediately adjoining the piers and abutments, substituting therefor a suspending system from above; also in using the inherent stiffness of the arch ribs themselves as caulilevers, aided, if found necessary, by temporary "guys" from the piers and abutments, to support the derricks and stages from which to project forward the successive sections of the ribs. Space will not admit of our entering further upon this subject at present, but we shall return to it again upon a future occasion, as it is one of extreme novelty and well deserving of further notice.

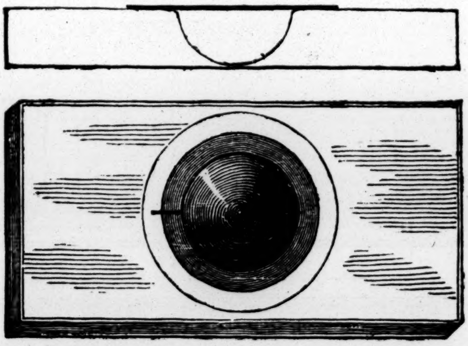
Channel Steamer.—**Whilst the various schemes for rendering communication between this country and the Continent are under consideration,—whether**

by means of a tunnel or otherwise,—Mr. Bessemer has recently introduced a new type of steam-vessel, the leading principle of which is to neutralise the wave-action of the sea to such an extent as that a portion of the ship—namely, the state cabin—shall remain always at rest, thus overcoming all the present incentives to sea-sickness. The pitching action of the vessel is proposed to be overcome by giving it a very low freeboard at either end, and driving it through the waves instead of allowing it to mount them. The rolling motion is overcome by suspending the saloon at each end, and at two intermediate points, upon steel axes, supported upon standards. To prevent the saloon from being affected by the oscillation of the vessel, or its equilibrium from being disturbed by the movements of the passengers, it is fitted with hydraulic gear, by means of which its position with respect to the vessel is placed under perfect control, an attendant, having a spirit-level before him, being enabled, by the manipulation of a single lever, at all times to keep the floor of the saloon horizontal. A detailed description of the apparatus to be employed would, however, occupy more space than we can afford to give to it here.

LIGHT.

Mr. D. S. Holman has contrived a slide for viewing bacteria, vibriones, and other low organisms, under the highest powers of the microscope. The slide consists of a central polished cavity, about which is a similar polished bevel; and from the bevel outwards extends a small cut, the object of which is to afford an abundance of fresh air to the living things within, as well as to relieve the pressure, which shortly would become so great—from the expansion of the liquid within—as to cause the destruction of the cover-glass. No special dimensions are stated for the central cavity. The bevel is usually $\frac{1}{8}$th inch in diameter (the engraving is two-thirds of natural size); the small canal is cut through the inner edge of the bevel or annular space outward, for the purpose named above. It is found upon enclosing the animalculæ, &c., that they will invariably seek the edge of the pool in which they are confined, and the bevelled edge permits the observer to take advantage of this disposition, for when beneath it the objects are within range of the higher power object-glasses. Another very important feature in the device is the fact that a preparation may be kept within it for days or weeks together without losing vitality, owing to the simple arrangement for supplying fresh air.

FIG. I.



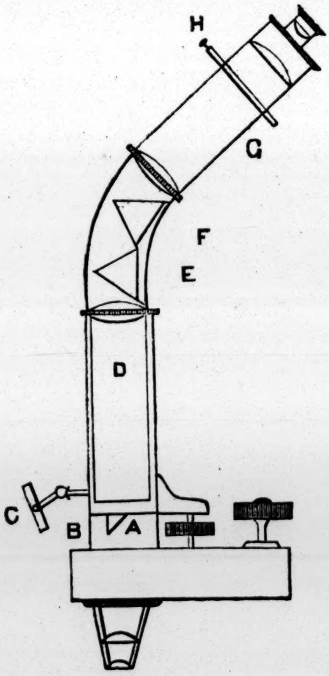
Dr. J. J. Woodward, in some remarks on the resolution of the nineteenth band of Nobert's plate, states that he has obtained the best results with objectives rather under-corrected as to colour. This entirely coincides with the practice of some of the best London opticians who have directed their attention chiefly to the perfect correction of the spherical aberration, and, knowing the impossibility of entirely correcting the chromatic aberration, have always left a small amount of colour, not only without injury to the performance of the combination, but with positive advantage.

Mr. F. H. Wenham has succeeded in constructing objectives with a single posterior as well as anterior lens, the only compound lens being the middle combination. With respect to the fitness of the high angle of aperture of the glasses at present in use for ordinary work, Mr. Wenham considers "that a $\frac{1}{20}$th of about 95°, accurately corrected, and having a long working distance, say $\frac{1}{80}$th of an inch, would be a valuable glass in the hands of a naturalist, enabling him to *see into* things instead of a mere surface observation of a few diatoms, for the sake of performing the feat of defining the difficult marking of some half-dozen of them,—and this is only what such a glass is at present used for."

Messrs. Powell and Lealand have constructed an objective of $\frac{1}{80}$th of an inch nominal focus; its angular aperture is 160°; the magnifying power is 4000 diameters with the A eye-piece, and it bears the B and C eye-pieces with no other detriment than a slight loss of light; it works well through a cover of 0·003 inch. It was exhibited at a recent meeting of the Quekett Microscopical Club, and, notwithstanding the unfavourable conditions under which it was tried, it showed the Podura scale sharply, without colour and with abundance of light.

At the December meeting of the Royal Microscopical Society Mr. Gayer exhibited a micro-spectroscope of novel construction. The slit is placed in the lower part of the body at about an inch distance above the object-glass; the slit is adjusted by the usual contrivances, and a small right-angled prism, B, and mirror, C, supply the means of obtaining a second spectrum for comparison. The image of the slit is formed by the collimating lens, D, above which are mounted two prisms of 60°, E and F. Attached to the curved tube containing the prisms is the telescope, G, having all necessary arrangements for focussing, and also a micrometer, H. When the object to be examined is so small that there is any doubt as to its image filling the slit, the spectroscope is removed, and the ordinary draw-tube with erector substituted; the slit can then be viewed, and so adjusted as to include the whole of the object. The prisms are of such dispersive power as to distinctly split the D line. The points of novelty are the position of the slit and the employment of a telescope to view the spectrum, which of course allows of variations of magnifying power by changing the eye-piece. Mr. Gayer claims for this micro-spectroscope the advantages of increased light and greater dispersion than in the ordinary direct vision instrument placed over the eye-piece. The latter property is not altogether an unmixed gain, for although dispersive power is invaluable for separating the bright lines of incandescent gases, the conditions required for the work of the micro-spectroscope are very different; the majority of absorption-bands are by no means sharp or well-defined at their edges, and are, as a rule, best seen with prisms of comparatively low dispersion, as more powerful instruments only thin out the bands and render their boundaries less evident.

FIG. 2.



HEAT.

Prof. Volpicelli, in "Poggendorff's Annalen," says—It has been asserted that a lowering of temperature is produced when air, which has been compressed in a vessel, is allowed to stream out against the surface of a thermopile. To test this assertion I compressed air in a cylindrical vessel to four atmospheres, and, after the heat of the compression had disappeared, I allowed the air to stream against a thermopile, which was connected with a reflecting galvanometer. Three different results appeared. If the commencement of the air-stream was pretty near the surface of the pile there was elevation of temperature, if it was somewhat distant from the surface the temperature fell, and at a point intermediate there was no change of temperature—the image reflected from the needle was unmoved. These results may also be obtained if air is blown, with an ordinary bellows, against the surface of the pile; only in this case the rise or fall of temperature is less marked, owing to the smaller compression of air. I also obtained the results, though in still less measure, with a centrifugal ventilator. These three results are quite in accordance with the new thermodynamic theory. In the experiments the causes of variation of temperature are of three kinds:—One consists of the destruction of the *vis viva* of the air, or *external* work; a second consists of *internal* work, done by the air molecules which become condensed in the pores of the metal of the pile; and the third of external work, done by the molecules as they expand in their course. The two first cause an elevation, the third a lowering of temperature. It is thus seen how one or other of the three above-described results is produced, according as the effects of the two first causes are greater or smaller than the opposing effect of the third cause, or equal to it. Remove the source of the air-stream and you have, first, a zero point of increase of temperature, then a decrease of temperature. Remove still further, and you come to a zero point of increase of temperature. From this is to be inferred that between these two distances (corresponding to the two zeros) there is a maximum of decrease of temperature which the galvanometer indicates. If it were possible to drive the air against the pile without compressing it, and, therefore, without expansion taking place, the two first causes only would operate and there would be heat produced. But these conditions are unattainable. In order to show to a large audience the transformation of destroyed *vis viva* into heat, I suspended a ball of phosphorus near a wall, and standing about 10 metres off, blew the ball with a pair of bellows against the wall. The ball was set on fire when it struck, not in its passage through the air. In another experiment I let a solid body fall on the surface of the thermopile, the latter being connected with a reflecting galvanometer. The reflected image was then seen to move several degrees over the scale, indicating elevation of temperature. This experiment is quicker and more simple than that sometimes performed in which a body is allowed to fall several times from a certain height on a hard substance, and then applied to the pile.

ELECTRICITY.

Dr. G. Robinson has recently patented a new method of sawing timber. It consists in applying a platinum wire, heated to redness or whiteness by an electric current, to the trees or wood which are to be severed much in the same manner as it has hitherto been employed in removing tumours from the human subject. By fitting the wire with handles so as to be able to guide it in any direction the most intricate fretwork can be cut.

M. J. Jamin has contributed to the French Academy of Sciences a paper, in which he shows that magnetism may be condensed in a manner similar to electricity. Having for some special purposes had a large horseshoe magnet made, consisting of ten laminæ of perfectly homogeneous steel, each weighing ten kilogrammes, he suspended it to a hook attached to a strong beam, and having wound copper wire round each of the legs, which were turned downwards, he put the latter into communication with a battery of fifty Bunsen's elements, by which means the horseshoe might be magnetised either positively or negatively at pleasure. The variations were indicated by a small horizontal needle situated in the plane of the poles. There was, further, a series of iron plates, which could be separately applied to each of the laminæ, Before attaching any of the latter, the electric current was driven through the apparatus for a few minutes and then interrupted, whereby the magnet acquired its first degree of saturation, marked by a certain deviation of the needle. One of the iron plates (usually called "contacts") was then put on, and it supported a weight of 140 kilogrammes. A second trial was now made; and the current having been passed through again for a few seconds, it was found that the horseshoe would support 300 kilogrammes, instead of 140. The number of contacts being now increased to five, which together in the natural state supported 120 kilogrammes, it was found after the passage of the current that they could support the enormous weight of 680 kilogrammes, which they did for the space of a week. No sooner, however, were the contacts taken off than the horseshoe returned to its usual permanent strength of 140 kilogrammes. This tends to show that magnetism may be condensed like electricity for a short period.

Zöllner has ascribed the electric currents of the earth to the motion of incandescent molten masses beneath the crust, which generate currents in the direction of their motion. He has also stated that all current movements of fluids, especially when in contact with solid bodies, are to some extent accompanied with currents of electricity, which have the same direction as the fluids themselves. To prove this he inserted the ends of the copper wires of a very delicate galvanometer just within the wall of a caoutchouc tube, through which a stream of water was passing; this caused a deflection of several degrees on the galvanometer scale, thereby indicating the existence of an electric current, whose direction was that of the water. The greater the distance between the ends of the wires, which may be replaced by metallic plates, the stronger the deflection of the needle. Beetz, while recently repeating Zöllner's experiment, obtained similar results, but found that the currents have a much simpler origin. The needle is deflected so long as the reservoir in which the water falls is not isolated. The metal, the stream of water, and the reservoir, form a voltaic element, whose current it is that deflects the needle. By filling the reservoir, and dipping the free end of the tube, also filled into it, the current is observed though the water be shut off, nor does any change take place when the tap is opened. By simply inverting the position of the tube, the direction of the current is reversed; this is observed to be the case with or without a flow of water. If the reservoir is isolated, no current is formed, whether the water be allowed to flow or not. When the tap and reservoir are of zinc, no current is produced with or without a flow of water, and with or without isolation of the reservoir. Therefore, according to these observations, no electricity is generated by a stream of water.

TECHNOLOGY.

Professor Chevreul has made a series of experiments on the stability of dyes imparted to silks, damasks, and fabrics used in furnishing, The blue colours produced by indigo are stable; Prussian blue resists moderately the action of air and light, but not of soap; scarlets and carmines produced by cochineal and lac-dye are fast; the most stable yellows on silk are produced by weld.

M. Dubrunfaut, during the siege of Paris, devised an artificial milk, made by dissolving one ounce and a half of sugar in a quart of water, adding an ounce of dry albumen (from white of eggs), and 15 to 30 grains of soda crystals, and then making an emulsion with it by means of from one ounce and a half to two ounces of olive oil. As the war progressed gelatine was substituted for albumen, and slaughterhouse fats, purified by melting at 150°, for the olive oil. One firm made by the latter process 132,000 gallons of milk daily for Paris consumption.

M. E. Daniel states that painting in oil may be executed upon tin-foil spread out upon a smooth surface, such as glass, the latter having first been moistened to aid the laying out of the tin, and to maintain it in its position. The painting, when dried and varnished, can be rolled up like ordinary paper-hangings, from which it essentially differs in possessing all the variety of tones and colouring that oil paintings admit of. The tin groundwork constitutes a water-proof protection, and, on account of its great flexibility, will follow the various mouldings and contour of the object to be ornamented. To the latter should be applied a hydrofuge mixture; it will then be ready for the decorator. Ordinary gilding may be replaced by this method, as the gold can be applied in the workmanship and the gilt tin fixed afterwards. The advantage of gilt tin over gilding on other metals is, that it is inimical to oxidation; whereas it is known that gilding upon other metals, and notably upon zinc, deteriorates rapidly.

A quantity of tin in ingots was, during a severe frost, sent from Rotterdam to Moscow. On arriving it was found to be in a coarse crystalline powder, which could not be fused into the ordinary condition of tin; for, on the application of heat, it was almost entirely converted into oxide of tin, the appearance of which closely resembled sulphide of molybdenum. On being analysed it was found to contain 99·7 per cent of pure tin, the remainder being lead and iron. The cause of the change was attributed to the long-continued vibration it underwent at so low a temperature. Similar conditions have been known to render wrought-iron extremely brittle, and its texture crystalline and granular.

M. Marion, of Paris, has devised a method of photographic printing; it consists in impregnating paper with ferroprussiate, which renders it sensitive to light. The drawing, which is made on tracing paper, is laid upon the sensitive paper as a negative and exposed to light, after which the sensitive paper is washed in water; the copy is then found to be produced on it in white line on a blue ground, which may be changed to black, the drawing still remaining white by using a tannin solution.

The French Mint has recently coined, for the Bank of France, 6000 or 7000 lbs. of Australian Gold, known as "brittle." All the pieces have been found to be easily broken, and have, therefore, to be re-melted. The defect is attributed to the presence of a small percentage of antimony and arsenic, extremely difficult of removal. These elements are known to produce a similar effect in all metals or alloys that are subject to the molecular changes induced by the pressure and heat developed under the action of the dies in the coining press.

Owing to the fact that water-glass is gradually dissolved out of wood while chloride of zinc is volatile at the temperature at which wood ignites, Dr. Sieburger proposes as a fire-proof paint for woodwork the following:—Two coats of a hot saturated solution of 3 parts alum and 1 part ferrous sulphate are first applied and allowed to dry. The third coat is a dilute solution of ferrous sulphate, into which white potter's clay is stirred until it has the consistency of good water-colours. Another method is to apply hot glue-water as long as it is absorbed into the pores of the wood. A thick coat of boiled glue is then applied, and while fresh is dusted over with a powder composed of 1 part sulphur, 1 part ochre or clay, and 6 parts of ferrous sulphate.

M. Tatro, the inventor of a process for purifying petroleum, states that by adding from 2 to 4 per cent of sulphuric acid, and 4 to 6 per cent of dry lime, agitating the oil with this mixture, and proceeding with the distillation, a larger proportion of burning oil is produced.

Palmetto leaves have recently been shipped from Savannah to England for the purpose of testing their value in the manufacture of paper.

CHEMICAL SCIENCE.

Having ascertained that furfurol is formed when wood is heated with water to an elevated temperature and pressure, Mr. Greville Williams, F.R.S, explains the method by which he found it to be produced by the action of high-pressure steam on the same substance. The apparatus employed is shown in the engraving. A A is a bronze autoclave, made in one piece, and of great strength. Before being employed it was tested by means of a hydraulic pump, and was found to withstand a pressure of 500 lbs. to the inch without leakage. Before using the instrument a ring of vulcanised india-rubber was placed between the autoclave and its cover, B B. The screw, C, which serves to keep down the cover, is forced home by means of a wrench applied at L. The arms, D D, serving to support the screw, are affixed to projections on the autoclave, by movable steel pins inserted at E E. A screw-tap, F, enables the product to be distilled over at the conclusion of the operation. The pressures are indicated by the gauge, G. A cylinder of perforated metal, H, is used to contain the substance to be experimented upon; which, in this case, was pine sawdust. The shelf, I, also perforated, prevents contact of the sawdust with the water, the level of the latter being shown at K K. The water and the charge of sawdust having been introduced, the apparatus was immersed to about half its depth in an oil-bath, the temperature being carefully regulated by means of a thermometer. The oil-bath was then heated until the gauge indicated a pressure of 100 lbs. to the inch; this pressure was maintained in some experiments for three, and in others for four hours, the average temperature of the oil-bath being about 198° C. The apparatus having been allowed to cool until the pressure had completely gone down, was then connected with a condensing arrangement. The screw, F, was then loosened, and heat was applied to the oil-bath until about three-fourths of the water present had distilled over. The distillate was strongly acid to test-paper, and smelt decidedly of furfurol, mixed with an empyreumatic odour. On the addition of ammonia it acquired a yellow tint, and in a few hours deposited the characteristic crystals of furfuramide. The crude distillate, mixed with aniline and acetic or hydrochloric acid, instantly gave the magnificent crimson colouration indicative of furfurol. To prove that the crystalline precipitate with ammonia was really furfuramide, this was distilled with a very small quantity of hydrochloric acid; the distillate immediately gave the crimson reaction with aniline. The crystals, treated with acetic acid and aniline, also reacted in the same manner. The author next proceeded to ascertain whether wood would yield furfurol when distilled with water at normal pressures. He therefore distilled 100 lbs. of sawdust with 100 gallons of water, in a still heated by a copper steam coil: 20 gallons were distilled over. These 20 gallons were put into a small copper still, and the first 10 gallons received. These in their turn were rectified again, and two received. In spite of the concentration which these liquors had undergone, furfuramide was obtained on digestion with ammonia, and, in fact, they only contained minute traces of furfurol.

FIG. 3.